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This paper was co-authored with Dr. Charlotte Bartels, Prof. Dr. Sebastian Eichfelder and Dr. Carla Pöschel. All authors contributed equally to the preparation of the paper.

Article 2: *How does Bonus Depreciation Affect Real Investment? Effect Size, Asset Structure, and Tax Planning*

This paper was co-authored with Prof. Dr. Sebastian Eichfelder and Prof. Dr. Kerstin Schneider. Sebastian Eichfelder and Kerstin Schneider prepared the first draft of the paper (published under the title *How do tax incentives affect business investment? Evidence from German bonus depreciation* as arqus Discussion Paper No. 231), while I contributed to the content revision, updating and final formatting to prepare for submission.

Article 3: *Steuersatzsenkungen versus Sonderabschreibungen: Was ist die bessere Strategie zur Förderung der Standortattraktivität Deutschlands?*

This paper was co-authored with Prof. Dr. Sebastian Eichfelder, Mike Kluska and Juliane Selle. We contributed equally to the preparation of the paper. The version of the paper included in this dissertation is published in *Steuer und Wirtschaft* (`https://doi.org/10.9785/stuw-2022-990308`). For ease of reading, the same formatting as in the other chapters has been chosen (except for the language) instead of the journal formatting. An earlier version of this paper is available under the title *Senkung der Unternehmenssteuerlast versus Förderung von Investitionen: Was ist die bessere Strategie zur Förderung der Standortattraktivität Deutschlands?* published as arqus Discussion Paper No. 263.

Article 4: *How to Account for Tax Planning and Its Uncertainty in Firm Valuation?*

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Article 5: *Wage Response to Corporate Income Taxes: A Meta-Regression Analysis*

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Other publications

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- Eichfelder, S., J. Knaisch und S. Nicolai (2021): Orientieren sich die steuerlichen Reformvorschläge der Parteien zur Bundestagswahl 2021 am Leistungsfähigkeitsprinzip?, in: *Deutsches Steuerrecht*, S. 2168-2177.

- Eichfelder, S., J. Knaisch (2020): Hohe Bürokratiekosten durch temporäre MwSt-Senkung, in: *Der Betrieb Spezial: Bewältigung der Corona-Krise* 19/2020, S. 2-4.

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Chapter 1

Preface

This dissertation contains five essays on the relationship between business taxes, real investment, and wages. Business investment is a significant factor for the growth and welfare of an economy (e.g., Hall and Jorgenson 1967; Auerbach and Hassett 1992; Goolsbee 1998). At the same time, the ratio of the production factors capital and labor is an important indicator for the efficiency of the overall economic production process (e.g., Arrow et al. 1961). The effects of business taxes on firms' investment behavior as well as on employees' wages are therefore a fundamental area of tax research. While some of the previous literature has found strong effects of business taxes on investment (e.g. Cummins et al. 1994; Chirinko, Fazzari, and Meyer 1999; House and Shapiro 2008; Bond and Xing 2015; Ohrn 2018) and wages (e.g., Arulampalam, Devereux, and Maffini 2012; Hassett and Mathur 2015; Fuest, Peichl, and Siegloch 2018), there are still a number of open questions and unsolved issues.

First, a large part of the literature does not sufficiently consider the relevance of aggressive tax planning (e.g., Slemrod 1995; Rünger 2019; Jacob 2022). This risks overestimating *real* tax effects, since these might be estimated as the sum of the effects of aggressive tax planning (such as profit shifting and investment in financial assets) and real investment. Second, the literature produces heterogeneous results, depending on the investigated tax policy. While studies that examine tax effects based on the user cost of capital find relatively moderate elasticities between 0.2 and 1 (e.g., Auerbach and Hassett 1992; Cummins et al. 1994; Caballero et al. 1995; Chirinko, Fazzari, and Meyer 1999; Dwenger 2014; Bond and Xing 2015; Giroud and Rauh 2019; Mutti and Ohrn 2019; Melo-Becarra, Mahecha, and Ramos-Ferrero 2021), studies on the impact of tax incentives such as bonus depreciation programs find much stronger tax effects (elasticities between 6 and 14, see, e.g., House and Shapiro 2008; Zwick and Mahon 2017; Ohrn 2018; Maffini, Xing, and Devereux 2019). Third, there are only few studies that identify tax effects for different types of firms (e.g., Zwick and Mahon 2017; Fuest, Peichl, and Siegloch 2018), especially at the micro level of business establishments. Finally, the question of who ultimately bears the burden of business taxes, in particular the corporate income tax (CIT), capital or labor (CIT incidence), is an important issue for policy makers due to its implications for the progressivity and distributive fairness of a tax system (Auerbach 2006). Yet, despite its policy relevance there is no consensus on the magnitude of the impact (Organisation for Economic Co-operation and Development 2015), and the specifications, samples, and policy implications of studies vary widely and face a variety of methodological challenges (e.g. Gravelle and Smetters 2006; Auerbach 2006; Harberger 2008; Clausing 2013).

Chapter 2 of this dissertation examines firms' real investment responses by using a generalized difference-in-differences approach based on 10,702 changes in the German Local Business Tax (LBT) rate from 1995 to 2016. In this institutional framework, the tax variation only arises from tax *rate* changes determined by municipalities, as the tax base is set at the federal level. Thus, the tax base is determined according to the same rules for all establishments, while the tax rate varies. Unlike most previous studies that use aggregate country or accounting data, this paper relies on a detailed and mandatory establishment-level investment survey (*AFiD* panel) collected for an administrative panel of all German manufacturing establishments with at least 20 employees (about 43,000 per year). For aggregate (total) real investment, the paper finds no statistically significant average responses, neither at the extensive nor at the intensive margin. Accounting for investment type and firm heterogeneity shows that LBT rates exert a significant impact on the likelihood of investments (extensive margin) (a) with long depreciation periods (land and buildings), (b) of large establishments ($\geq$ 250 employees or $>$ €50 million in sales), (c) of highly productive establishments, and (d) of establishments belonging to multi-establishments firms. In addition, the paper finds evidence for investment shifting within multi-establishment firms at the intensive margin, indicating that opportunities for tax planning are used by firms.

Chapter 3 exploits an exogenous variation in regional tax regulation in East Germany (*Development Area Law*, DAL) that affects the tax *base* by providing a bonus depreciation of up to 50%, relying on the same administrative data source as in Chapter 2. In contrast to tax rate changes, the baseline results suggest that the bonus depreciation program increased real gross investment by 16.0% to 19.9% on average. This overall effect is especially driven by additional investment in buildings (76.6% to 92.3%) and land (108.0% to 121.3%), which have the longest regular depreciation periods in the absence of bonus depreciation. The impact on equipment investment is much smaller (7.3% to 10.5%). Thus, firms not only increased their real investment, but also adjusted their asset structure in response to the tax incentive. In terms of firm heterogeneity, Chapter 3 finds similar results to Chapter 2: a stronger response for firms with more than one establishment and for large firms, suggesting that tax planning opportunities (firms with multiple establishments) and relatively low tax planning costs (large firms) amplify the effect of bonus depreciation on investment.

Chapter 4 compares the effects of two alternative tax policy measures – a permanent reduction in the statutory tax rate for firms or a temporary bonus depreciation – on Germany's attractiveness as a business location. In a conceptual analysis, the paper considers the empirical literature on location decisions, investment and foreign direct investment (FDI), as well as business surveys and internationally accepted indices of corporate location attractiveness. Business surveys and indices suggest a moderate direct impact of corporate taxation on business location attractiveness. In addition, indices such as the Global Competitiveness Index point to weaknesses of Germany in other more relevant location attractiveness factors (demographics, digitalization, infrastructure). The paper argues that tax policy should focus on promoting investments that compensate for these weaknesses (indirect tax effects). In line with the results of the previous two chapters, bonus depreciation seems to be the more targeted and effective policy instrument to increase the attractiveness of Germany as a business location if compared to statutory tax rate changes.

While the previous chapters have considered that tax planning strategies of firms could distort common measures of investment (e.g., FDI stocks), Chapter 5 investigates whether tax planning is effective to increase shareholder value. I examine if and how investors value tax planning of publicly listed firms in Germany and compare two approaches from recent literature on how to account for tax planning and its uncertainty. Thus, I analyze the separate view of Drake, Lusch, and Stekelberg (2019) and the composite view of Jacob and Schütt (2020), with an emphasis on measurement issues and firm heterogeneity. The results suggest that investors indeed deem tax planning as value-relevant, and that they do not only care about the level of tax planning, but also it's uncertainty. The analyses imply that higher uncertainty-adjusted tax planning amplifies the positive association between pre-tax income and firm value. However, the economic magnitude of this association depends on measurement approaches and control settings. Furthermore, the positive value implication is particularly pronounced for firms with low leverage whose debt tax shield and debt overhang are relatively small.

Finally, Chapter 6 presents a meta-regression analysis (MRA) on the responsiveness of wages to corporate taxes. The MRA quantitatively reviews the growing empirical incidence literature, which points to a potentially substantial shift of the corporate tax burden to workers. While most studies report a wage-reducing effect of corporate taxes, the paper's results suggest that estimates with positive values are published less frequently than they should (publication bias). After accounting for this bias, there is no evidence of a statistically significant average relationship between wage rates and corporate taxes. Multiple regression analyses document that the tax variable, the econometric method, the type of tax variation, and the underlying time and country coverage of the studies drive the heterogeneity among the reported estimates. Specifically, wage incidence estimates seem to be larger when tax-base-related determinants of the CIT burden (e.g., firms' tax planning strategies) are acknowledged by studies. The implied best-practice estimates suggest that the tax elasticity of wages in emerging markets is systematically larger and smaller when tax changes are used at the subnational level for identification.

Two aspects run as a common thread through this dissertation: i) The theoretical background of CIT incidence (Chapter 6) partly builds on investment and capital formation channels (Chapters 2 to 5), and ii) The dependency of empirical results on measurement and specification choices is emphasized in all chapters. Related to this, the role of corporate tax planning is addressed in each paper – indirectly in Chapters 2, 3, 4, and 6 by discussing and attempting to deal with the potential measurement error caused by tax planning in commonly used investment and tax burden measures (e.g., De Mooij and Ederveen 2008; Feld and Heckemeyer 2011), and directly in Chapter 5 by examining how investors value firms' tax planning.

Chapters 2, 3, and 4 are particularly closely linked, as Chapter 4 builds in part on the findings of the previous two chapters (tax rate versus tax base changes and their impact on real investment). Chapter 5 is also concerned with investment, particularly how tax planning is valued by shareholders. All of these issues are relevant determinants of CIT incidence investigated in Chapter 6. There are at least two theoretical incidence mechanisms: the direct and indirect incidence. While the direct channel is based on wage bargaining models (e.g., McDonald and Solow 1981; Arulampalam, Devereux, and Maffini 2012), the indirect channel builds on a general equilibrium model for an open economy in which a higher corporate tax rate leads to capital outflows to lower-taxed jurisdictions, a lower capital stock, and thus lower labor productivity in the high-tax country, which ultimately depresses wages (e.g., Harberger 1962; Harberger 1995; Harberger 2008; Auerbach 2006): $CIT/TaxPlanning \rightarrow Capital \rightarrow LaborProductivity \rightarrow Wages$. Thus, the investment response to tax policy is a key determinant in understanding CIT incidence.

Taken together, the papers in this dissertation give rise to three key insights: i) Tax policy measures that target the tax base might be more effective in fostering real investment in comparison to tax rate changes, ii) It seems important to carefully measure real investment aside from "artificial" tax avoidance strategies (e.g., profit shifting, financial investments), iii) The impact of business taxes on real investment and wages appear to be heterogeneous with respect to both the type of investment and the type of firm. Solely relying on average responses might therefore result in misleading conclusions.

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Chapter 2

How do Business Tax Rates Affect Real Investment? The Role of Depreciation and Firm Characteristics

How do Business Tax Rates Affect Real Investment? The Role of Depreciation and Firm Characteristics

Abstract

We investigate the effect of local business tax (LBT) rates on real business investment at the extensive and intensive margin. Unlike most prior studies using aggregate country or accounting data, we rely on a detailed and mandatory investment survey at the establishment level collected for an administrative panel of all German manufacturing establishments (about 43,000 per year). We adopt a generalized difference-in-differences approach drawing on 10,702 LBT rate changes from 1995 to 2016. We find no significant average investment responses, neither at the extensive nor the intensive margin. Taking account of investment type and firm heterogeneity reveals that LBT rates exert a significant impact on the probability of investments (extensive margin) (a) with long depreciation periods (land and buildings), (b) of large establishments (≥ 250 employees or $>$ €50 million turnover), (c) of high-productivity establishments, and (d) of establishments belonging to multi-establishment firms.

JEL classification codes: G11; H25; H32; M41

Keywords: business taxation, user cost of capital, tax elasticity, real investment

2.1. Introduction

Policymakers widely use tax-based incentives to spur investment and stimulate economic growth. Tax policy has been at the center of emergency measures during the Covid-19 pandemic, and it is now as countries face a significant deterioration in public finances (OECD 2021). Yet, empirical tax research is still in disagreement on how taxes affect business investment.

An important but often overlooked challenge for estimating investment responses to taxes is the measurement of investment. Studies estimating the semi-elasticity of investment to taxes are usually based on FDI, accounting, or administrative tax data. FDI data capture both real business activity and financial investments. But financial investments used for tax avoidance might exceed adjustments of real business activity, so that FDI data may produce upward biased responses (e.g., Slemrod 1995).[1] Accounting data-based elasticity estimates may also be upward biased because of firms' incentives to avoid taxes.[2] Studies using administrative tax data find large (e.g., Zwick and Mahon 2017) to very small or zero tax elasticities of investment (e.g., Yagan 2015; Harju, Koivisto, and Matikka 2022).

This study introduces a rich panel of about 43,000 German manufacturing establishments for the years 1995 to 2016 that allows estimating the impact of business tax rates on real investment activity (land, buildings, and equipment) at the extensive and intensive margin. AFiD (*Amtliche Firmendaten in Deutschland*) is an administrative dataset provided by the German federal statistical office, which entails a detailed and mandatory investment survey, recording establishment-level information on annual investments in land, buildings, and equipment, as well as turnover, total wages, and the number of employees. Such data will be unaffected by financial investments and tax avoidance schemes, which have likely biased many previous tax elasticities of investment. Further, we observe investments directly compared to accounting data-based studies that have to infer investments from changes in the capital stock. We match establishments with the local business tax (LBT) rate that both corporations (65% of the sample) and pass-through

[1]Studies proxying investment with FDI typically find high semi-elasticities of (effective) tax rates ranging from -2.5 to -3.3 (De Mooij and Ederveen 2003; De Mooij and Ederveen 2008; Feld and Heckemeyer 2011). The meta-study of Feld and Heckemeyer (2011) finds a significantly lower FDI elasticity for studies that use accounting data to control for financial investments. The median elasticity of -2.55 decreases to -0.57 if one deducts the partial coefficient of -1.98 for firm assets.

[2]Elasticities based on capital stocks from accounting data are lower than suggested by FDI studies and range from -0.25 to -1 (e.g., Auerbach and Hassett 1992; Cummins et al. 1994; Caballero et al. 1995; Chirinko, Fazzari, and Meyer 1999; Bond and Van Reenen 2007; Dwenger 2014; Bond and Xing 2015). As documented by the literature on tax-induced earnings management and conforming tax avoidance, firms use discretion in accruals like depreciation to reduce taxable income. Since lower profits imply lower book values and equity, such tax avoidance behavior will reduce fixed and total assets, especially in high-tax jurisdictions and periods (e.g., Scholes, Wilson, and Wolfson 1992; Sundvik 2017; Badertscher et al. 2019; Eichfelder et al. 2021).

entities (35% of the sample) are liable to.

We adopt a generalized difference-in-differences (DiD) approach drawing on 10,702 LBT rate changes in Germany (i.e., 9,839 hikes and 863 cuts between 1995 and 2016) using establishments as the most granular possible level of identification. Our identifying tax variation is similar to the study of Fuest, Peichl, and Siegloch (2018) who analyze the corporate tax incidence on wages. While the majority of German LBT rate changes were tax increases, this only partially compensated the general downward trend in German business taxation.[3] Our event study design regressions provide evidence of a common trend between treated establishments (i.e., with a tax rate change in the current period) and untreated control establishments (i.e., without a tax rate change in the current period). Given that ca. 27% of the establishments in our panel belong to multi-establishment firms, we also estimate the share of the response due to capital reallocation from high- to low-tax establishments.

Our identifying variation allows isolating the impact of tax rate changes from tax base changes. In the German LBT system, tax rates are set by the municipality, and the tax base is set by federal legislation. Thus, the tax base is identical for all municipalities in a given year, and we are able to identify precisely the impact of tax *rate* changes on investment. This is different from international comparison studies (e.g., De Mooij and Ederveen 2003; De Mooij and Ederveen 2008; Feld and Heckemeyer 2011; Bond and Xing 2015) and the US state corporate income tax (e.g., Giroud and Rauh 2019), where variations in tax rates are likely correlated with variations in tax bases.

Our core result is that investment responses are extremely heterogeneous across investment goods with different depreciation lengths and across firms. In our baseline specification, we obtain negative coefficients that are not statistically different from zero, confirming the average results from Harju, Koivisto, and Matikka (2022). The small baseline coefficient implies a semi-elasticity for the investment probability of less than -0.1. Nevertheless, firms are very responsive in their decision to invest in land and buildings (semi-elasticities for investment probability of ca. -3) but almost unaffected in their equipment investments (semi-elasticities for investment probability is smaller than -0.1).

Our contribution to the existing literature is three-fold. First, we contribute to the emerging literature emphasizing the heterogeneity in investment responses (e.g., Edgerton 2010; Zwick and Mahon 2017; Eichfelder and Schneider 2018). Taking account of investment type and firm heterogeneity reveals that LBT rates exert a significant impact on the probability of investments (extensive margin) (a) with long depreciation periods

[3] Due to tax reforms in the years 1999, 2001, and 2008, tax rates on retained profits of corporations decreased from 57.0% in 1995 to 29.8% in 2008. Regarding natural persons, business tax rates decreased from 58.8% to 47.4%. For calculating these rates, we assume a standard LBT multiplier of 4, the maximum corporate income tax rate and the maximum personal income tax rate, including the solidarity tax surcharge.

(land and buildings), (b) of large establishments ($\geq$ 250 employees or $>$ €50 million turnover), (c) of high-productivity establishments, and (d) of establishments belonging to multi-establishment firms. Our evidence suggests that investments with higher burdens resulting from tax rate changes (land, buildings, highly productive establishments), lower compliance and planning costs (large establishments, multi-establishment firms), and more tax avoidance opportunities (multi-establishment firms) react more strongly to tax rate changes.

Second, we add evidence to the literature documenting lumpy investments. We find statistically significant tax effects for investments almost exclusively at the extensive margin but not at the intensive margin. Stronger responses at the extensive margin result from investment adjustment costs, irreversibility of investments, the indivisibility of investments, and other factors of investment decisions such as the replacement of investment (e.g., Caballero and Engel 1999; Cooper and Haltiwanger 2006; Chen et al. 2022).

Third, we highlight the relevance of the tax base and the length of the depreciation period. Theory predicts that shorter depreciation periods moderate the role of tax rates for investment decisions (e.g., Hall and Jorgenson 1967; Devereux and Griffith 2003). Previous studies that estimate business tax elasticities based on either cross-country differences in effective tax rates or user costs of capital lump together the effects of tax rate and tax base changes. Yet, empirical evidence suggests that temporary adjustments of tax bases might have a much larger effect on investment than persistent adjustments of the tax rate. Moderate responses to tax rate changes (e.g., Yagan 2015; Giroud and Rauh 2019; Harju, Koivisto, and Matikka 2022) contrast with large responses to tax incentives like bonus depreciation (e.g., House and Shapiro 2008; Zwick and Mahon 2017; Eichfelder and Schneider 2018; Curtis et al. 2021). On average, we only find statistically significant effects for investment goods without regular depreciation (land) and with longer depreciation periods (buildings). This is in line with the literature on cash flow taxation (e.g., Auerbach and Bradford 2004; Auerbach et al. 2017) arguing that business taxes will not distort the user costs of capital in case of an immediate write-off (Cohen, Hansen, and Hassett 2002). While depreciation periods for equipment are larger than zero in Germany, they seem to be sufficiently short to significantly reduce the impact of tax rate changes on investment decisions.

Our results imply that business tax *rates* seem to be less relevant for real investment than political debates and previous research—especially studies based on FDI data—suggest. Both the literature on agglomeration effects (e.g., Baldwin and Krugman 2004; Hühnerbein and Seidel 2010; Brülhart, Jametti, and Schmidheiny 2012; Luthi and Schmidheiny 2014) and indices of location attractiveness suggest a relatively limited relevance of business tax rates.[4] Instead, tax *bases* might be the relevant parameter, also from an

[4]The share of the Global Competitiveness Index of the World Economic Forum that is explained by

efficiency perspective. The shorter the depreciation period, the smaller and less distortive the impact of business tax rates on investment decisions.[5] This implication questions whether *rate-cut cum base-broadening* tax reforms (i.e., tax reforms that reduce rates but increase the tax base by reducing benefits from depreciation and other deductions) are efficiency enhancing. On the contrary, our evidence suggests that shortening depreciation periods and simultaneously raising tax rates might be efficiency enhancing.

The remainder of this paper is structured as follows. Section 3.2 introduces the institutional setting of the German LBT and our data set. Section 3.3 explains our conceptual approach. Section 2.4 outlines our identification strategy. Section 3.5 presents and discusses our estimation results. Finally, Section 3.6 concludes.

2.2. Institutional Setting and Data

2.2.1. German Local Business Tax

Profits of corporations in Germany are liable to the corporate income tax (CIT, *Körperschaftsteuer*), to solidarity tax surcharge (STS, *Solidaritätszuschlag*) on the CIT, and to the local business tax (LBT, *Gewerbesteuer*). Profits of pass-through entities (partnerships and sole proprietorships) are liable to the personal income tax (PIT, *Einkommensteuer*), STS on the PIT, and the LBT. We exploit changes in LBT rates within municipalities over time, affecting corporations, partnerships, and sole proprietorships. Federal legislation defines the LBT tax base as an adjusted taxable profit with certain add-backs and exemptions. Most importantly, interest, leasing costs, and rent (exceeding an exemption threshold) are only partially deductible from the tax base. Distributed profits and dividends are tax-exempt to avoid double taxation.

The LBT rate τ is a combination of a uniform basic rate θ_t (*Steuermesszahl*) and a local multiplier $m_{j,t}$ (*Hebesatz*). Municipality j sets the multiplier for each year t. In 2004, the German legislator introduced a minimum tax multiplier of 2 to limit aggressive tax competition and profit shifting among municipalities.

From 1995 to 2007, the LBT payment was a deductible business expense that reduced its own tax base. The basic rate θ in this period was 5%, and the lump-sum LBT

the tax burden, apart from compliance costs, is only 1.38% of the overall index, that is, the location attractiveness (own calculations based on World Economic Forum (2019), pp. 611). Similarly, low fractions of the relevance of business taxes result from the Ease of Doing Business Index of the World Bank (2.44%; own calculations based on World Bank Group (2020), pp. 78) or the World Competitiveness Yearbook of the International Institute for Management Development (2.5%; own calculations based on IMD (2020)).

[5] We might term this "taxation of target values". If the tax base is identical to the target value of the decision maker, taxation should not affect economic decision-making; see also Hall and Jorgenson (1967), Cohen, Hansen, and Hassett (2002), Giroud and Rauh (2019).

tax credit (9% of the adjusted LBT profits) for natural business owners did not depend on the LBT multiplier. In this period, the effective LBT rate is given by:

$$\tau_{j,t} = \frac{m_{j,t} \cdot \theta}{(1 + m_{j,t} \cdot \theta)} \quad \text{with} \ \ t \in [1995, 2007] \tag{2.1}$$

In 2008, the German Business Tax Reform (GBTR 2008) reduced the basic rate from 5% to 3.5% and abolished the tax deductibility of LBT payments. In addition, the LBT tax credit against the personal income tax depends on the LBT payment (no lump-sum credit) and is limited to a maximum multiplier of 3.8. The LBT rate since 2008 is given by:

$$\tau_{j,t} = m_{j,t} \cdot \theta \quad \text{with} \ \ t \in [2008, 2016] \tag{2.2}$$

The new regulations of the GBTR 2008 differentially affected the LBT burden for corporations and pass-through entities. On the one hand, the effective LBT burden on corporate taxpayers increased compared to pass-through entities after 2008 due to the abolition of the tax deductibility of LBT payments. On the other hand, the effective LBT burden for owners of pass-through entities (e.g., partners of partnerships or sole proprietors) remained zero or close to zero as these business owners can credit LBT payments against PIT payments. We use the GBTR 2008 as a quasi-experiment differentially affecting corporations and pass-through entities in Section 3.5.3 to check our main results.

Our analysis is on the establishment level. Multi-establishment firms with more than one establishment face a weighted average of LBT rates of the municipalities in which they operate, as the firm's taxable profits are apportioned to each municipality according to the wage share of each establishment (formula apportionment). A benefit of our setting is that the apportionment factor depends on wages and is thus not directly affected by business investment. Nevertheless, investment decisions may affect the allocation factor if investment decisions are related to employment decisions. We deal with this potential concern in two ways. First, we perform our baseline regression at the establishment level and thereby account for the fact that changes in investment decisions may indirectly affect the apportionment factor. For example, if investment increases go in hand with a higher share of wages, then the apportionment factor of the establishment increases as well. Second, we calculate a wage-weighted average LBT rate and perform our regression at the firm level in an alternative specification. Both approaches produce similar results (see Tables 2.4 and 2.5).

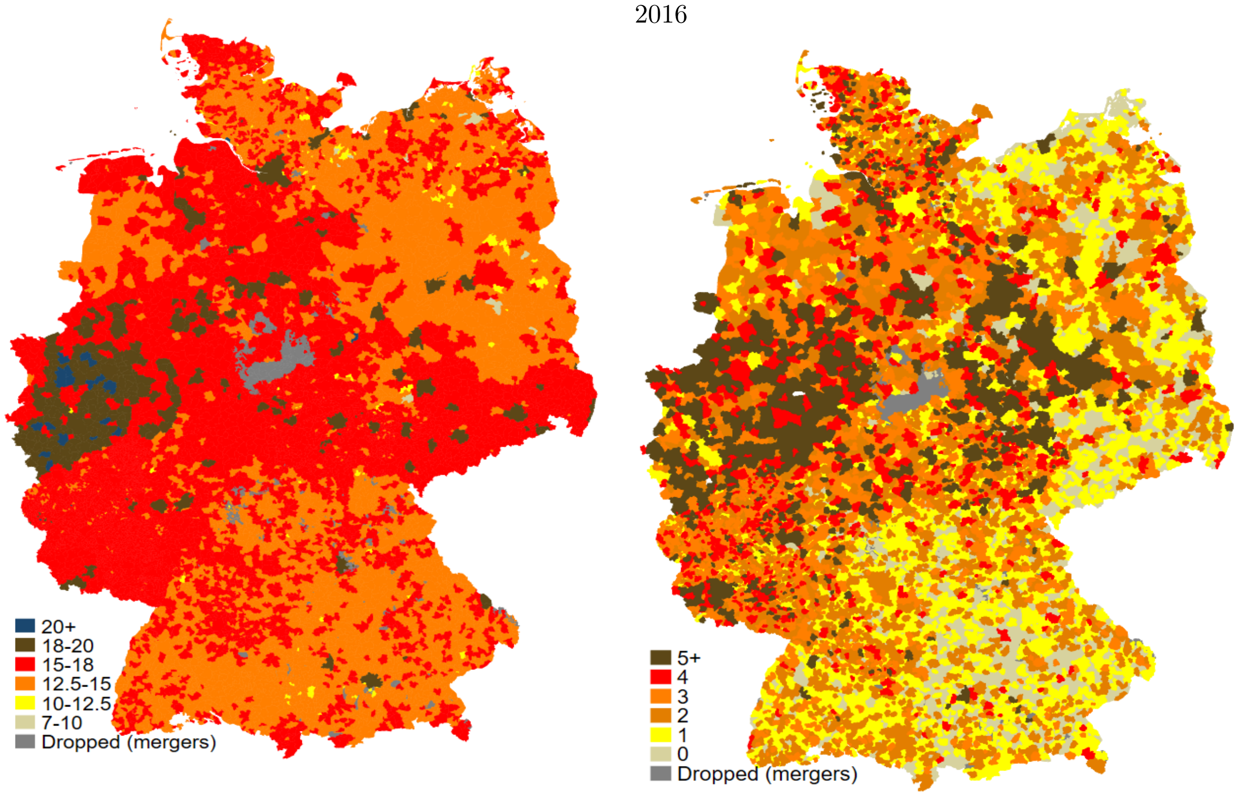
2.2.2. Variation in Local Business Tax Rates

To compute LBT rates, we collect LBT multipliers $m_{j,t}$ of all German municipalities for the years 1995–2016 from the statistical offices of the 16 German Laender (*Statistische Landesaemter*).[6] Panel A of Figure 2.1 shows the cross-sectional variation of LBT rates in 2016, and Panel B of Figure 2.1 presents the intertemporal variation from 1995 to 2016. Multipliers are higher in urban regions with high economic growth, such as Cologne (4.75 in 2016), Frankfurt (4.6 in 2016), Hamburg (4.7 in 2016), or Munich (4.9 in 2016), and North Rhine Westphalia due to its fiscal equalization scheme (Fuest, Peichl, and Siegloch 2018). The multiplier remained constant in 15% of the municipalities. 27% of the municipalities changed the multiplier once, 24% twice, 18% three times, and 16% four or more times.

Figure 2.1: Variation in LBT rates among German municipalities

a LBT rates in 2016

b Local multiplier changes per municipality, 1995–2016



Note: Panel A shows the distribution of LBT rates in percentage points in 2016. Panel B shows the number of multiplier changes per municipality from 1995 to 2016. Municipality boundaries are as of December 31, 2016. As some municipality identifiers changed due to mergers, we cannot identify the tax rates of these municipalities (grey areas) and exclude them. In sum, the figures cover multipliers for 11,011 municipalities.
Source: Own calculations are based on data from the statistical offices of the 16 German Laender; Maps: Esri Deutschland/BKG 2016.

[6] We can identify municipalities according to their boundaries as of December 31 of each year. Due to reorganizations, the number of municipalities varies over the years. We observed multipliers of 11,011 municipalities in 2016.

Table 2.1 gives an overview of the LBT rate variations across the municipalities in our sample ($N = 4{,}451$) with at least one manufacturing establishment. The number of municipalities with manufacturing establishments is considerably smaller than the overall number of municipalities (11,011). Note that about two-thirds of the German municipalities have less than 3,000 inhabitants (Statista 2020), out of which many will be hosting no manufacturing establishment. In sum, our data covers 10,702 LBT rate changes from 1995 to 2016. These split into 9,839 LBT rate hikes in 3,008 municipalities and 863 LBT rate cuts in 477 municipalities, showing that the majority (92%) of all LBT rate changes are tax rate hikes. The average LBT rate hike of 0.67 percentage points is smaller than the average LBT rate cut of 0.88 percentage points. Note that the higher number of tax hikes, which generated a slight upward trend of LBT rates, only partially compensated for the general downward trend in business taxes. For example, for a local multiplier of 4, the corporate tax burden on retained profits dropped from 57.0% in 1995 to 29.8% in 2016.

Table 2.1: Changes in LBT rates across municipalities with manufacturing establishments, 1995–2016

	LBT hikes	*LBT cuts*
Number	9,839	863
Municipalities	3,008	477
Mean	0.67	-0.88
Median	0.56	-0.53
Std. dev.	0.0067	0.0111

Note: This table provides an overview of LBT rate changes in 4,451 municipalities with at least one manufacturing establishment included in our sample.
Source: Own calculations are based on data from the German AFiD panel and the Statistical Offices of the 16 German Laender.

2.2.3. Data

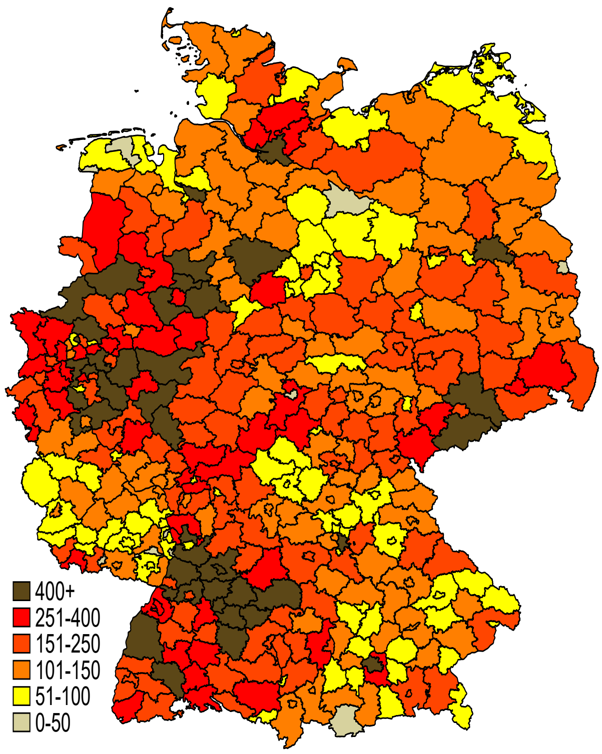
We combine the data on LBT rates with an administrative data set of German manufacturing businesses. We use the German AFiD panel (*Amtliche Firmendaten in Deutschland*) for manufacturing and mining industries from 1995–2016, which includes several mandatory business surveys conducted annually by the German federal statistical office. We use (1) the Investment Survey, (2) the Monthly Report, and (3) the Cost Structure Survey.[7]

[7]These are the Investment Survey for establishments of manufacturing, mining, and minerals (*Investitionserhebung bei Betrieben des verarbeitenden Gewerbes, des Bergbaus und der Gewinnung von Steinen und Erden*), the Monthly Report for establishments of manufacturing, mining, and minerals (*Monatsbericht für Betriebe des verarbeitenden Gewerbes, des Bergbaus und der Gewinnung von Steinen und Erden*), and the Cost Structure Survey for establishments of manufacturing, mining, and minerals (*Kostenstrukturerhebung bei Betrieben des verarbeitenden Gewerbes, des Bergbaus und der Gewinnung von Steinen und Erden*), respectively.

Manufacturing is still a highly relevant sector for the German economy generating more than 25% of total business profits subject to LBT in 2016 (Destatis 2021a).

The raw data set comprises 1,066,196 observations of business establishments between 1995–2016. Since our focus is on the manufacturing industry, we excluded 29,435 observations of firms and establishments in the mining industry. Finally, we dropped 219,389 observations with missing information on our explanatory variables. After these adjustments, our final sample contains 817,372 establishment-year observations for 77,133 establishments in 4,451 municipalities from 1995 to 2016. Our data covers municipalities with permanent establishments with at least 20 employees in the manufacturing industry. Given that most German municipalities are very small, our data contains almost half of the universe of German municipalities and virtually all economically relevant German municipalities. Figure 2.2 visualizes the number of establishments per county.[8] Most establishments are concentrated in West German states like North Rhine-Westphalia and Baden-Wuerttemberg.

Figure 2.2: Number of establishments per county, 1995–2016



Note: This figure illustrates the number of establishments (N = 77,133) per county from 1995 to 2016.
Source: Own calculations based on data from the German AFiD panel; Map: Esri Deutschland/BKG 2016.

Table 2.2 displays the list of variables that are collected by each of the three mandatory surveys. The Investment Survey and the Monthly Report cover the universe of German manufacturing establishments with at least 20 persons employed, including managers

[8] A visualization at the municipality level is impossible due to confidentiality restrictions.

and working business owners. They provide establishment-level data for capital investment of various types (land, buildings, equipment, and leasing), capital sales, turnover, total wages, and the number of employees. The Cost Structure Survey is only available for a subsample of 44.3% of our observations, because this survey is not a full but representative survey and gives information on the legal form, turnover, expenses, and earnings at the firm level.

Table 2.2: Surveys of the AFiD panel, 1995–2016

Survey	**Variables**	**Entity**
Investment Survey	capital investment (land, buildings, equipment, and leasing), capital sales	establishment
Monthly Report	turnover, total wages, number of employees	establishment
Cost Structure Survey	turnover, expenses (e.g., total wages, rent, interest, depreciation), number of employees, legal form	firm, available for 44.3% of the sample

Our dependent variable, *net investment*, is defined as gross investment less leased assets and capital sales.[9] AFiD allows us to distinguish between three major types of investment: land, buildings (e.g., factories where production takes place), and equipment (e.g., machines and instruments used for producing goods). These capital goods differ in their average useful life, which in turn, proxies their depreciation deductions. Land and buildings represent long-term capital goods with small depreciation deductions. In contrast, equipment investment represents a short-term capital good with large depreciation deductions.

We supplement the AFiD panel with data on regional economic conditions (i.e., GDP per capita, unemployment rate, population) at the county level[10] and the LBT multipliers, both provided by the Statistical Offices of the 16 German Laender. We define municipalities according to their boundaries as of December 31 of each year.

The AFiD data set has several important advantages in comparison to accounting data at the micro level. First, AFiD records real investments in different capital goods (land, buildings, and equipment) at the establishment level. This form of investment information not be affected by financial investments, conforming tax avoidance, and other forms of tax planning that can affect total assets and even fixed assets in accounting data (e.g., Scholes, Wilson, and Wolfson 1992; Badertscher et al. 2019; Eichfelder et al. 2021). The granularity of the data further allows us to identify tax rate variations at the

[9]We deduct leased assets because of their special accounting treatment. Depending on the leasing conditions, leased assets are either depreciated by the lessor or the lessee. Our data do not provide information to identify the leasing treatment. Table C.2.6 in Appendix shows that our results are robust to alternative investment definitions. In this table, we use gross investment less leased assets and net investment, including leased assets.

[10]The regional control variables are only available on the county level.

municipality level with 10,702 LBT rate changes and to differentiate between long-term and short-term investment goods with different depreciation deductions.

Second, AFiD provides data for the universe of about 43,000 German manufacturing establishments with 20 or more employees. Compared to accounting databases commonly covering publicly listed large firms, AFiD is more representative of the German industry. In 2016, 3.8 million firms were subject to the German LBT, out of which 58% were sole proprietors, 11% partnerships, 30% corporations, and 1% other legal forms (Destatis 2021a). Only 531 firms were publicly listed in 2016. The larger heterogeneity of firms in AFiD with respect to size and legal form (corporation vs pass-through entities) allows us to estimate heterogeneous responses and to test for potential biases arising from a focus on large and publicly listed corporations. Studies using data from large firms might overestimate average investment responses given that larger firms have more resources to consider tax incentives in their behavior (e.g., Ohrn 2019).

Third, AFiD provides information on investment flows as opposed to capital stocks provided by accounting data sets like Compustat or Orbis. Yagan (2015) and Zwick and Mahon (2017) are rare examples that also analyze investment flows, while, e.g., Giroud and Rauh (2019) analyze capital stocks. In the presence of measurement error, calculating investment from the change in capital stock between one year and the next will produce misleading results.

2.2.4. Summary Statistics

Table 2.3 provides summary statistics of our main variables for all establishments. All nominal variables are price-adjusted to the year 2015 using the German Producer Price Index for the manufacturing industry (Destatis 2021b).

About 87% of all establishments have positive investments between 1995–2016, with almost all of these establishments investing in equipment (87%) and a minority investing in buildings (19%) and land (3%). Figure B.2.2 in Appendix shows that this share is relatively stable between 1995 and 2016. Average net investment (gross investment less leased assets and capital sales) is €1,286 thousand per year, while the median is only €120 thousand, indicating a skewed distribution of net investment levels. Net investment of establishments amounts to ca. 3% of their turnover (€1.29 million/€40 million), on average. The average equipment investment (€1,148 thousand) is much larger than the average building investment (€149 thousand) and the average land investment (€9 thousand).

Our establishment sample consists of 65% corporations (vs 35% pass-trough enti-

Table 2.3: Summary statistics for the main analysis sample, 1995–2016

	N	Mean	Std. dev.	Percentiles		
				25th	50th	75th
Investment variables						
Gross investment probability (%)	817,372	87	34	100	100	100
Gross investment (1,000 €)	817,372	1,307	13,798	21	128	552
Land investment probability (%)	817,372	3	2	0	0	0
Land investment (1,000 €)	817,372	9	419	0	0	0
Building investment probability (%)	817,372	19	39	0	0	0
Building investment (1,000 €)	817,372	149	1,863	0	0	0
Equipment investment probability (%)	817,372	87	34	100	100	100
Equipment investment (1,000 €)	817,372	1,148	12,561	18	115	476
Leasing (1,000 €)	817,372	12	2	11	12	13
Capital sales (1,000 €)	817,372	21	272	0	0	3
Net investment probability (%)	817,372	87	34	100	100	100
Net investment (1,000 €)	817,372	1,286	13,781	18	120	527
Local business tax						
τ (%)	817,372	15	2	13	14	16
Business characteristics						
Large establishment ($\geq$ 250 empl.) (%)	817,372	11	31	0	0	0
Large establishment (> 50m turn.) (%)	817,372	12	33	0	0	0
Corporation (%)	362,441	65	48	0	100	100
Estab. of multi-establishment firm (%)	817,372	27	44	0	0	100
Establishment controls						
Capital stock (1,000 €)	817,372	5,793	43,883	798	1,513	3,529
Turnover (1,000,000 €)	817,372	40	410	3	7	21
Regional controls						
GDP per capita (€)	817,372	29,225	12,315	22,149	27,239	32,917
Unemployment rate (%)	817,372	8	4	6	8	10
Population	817,372	39,097	117,591	144	261	493

Note: Net investment is gross investment less leased assets and capital sales. Investment probabilities capture the percentage of establishments with positive investments in percentage points. Investment quantities capture the investment per establishment in €1,000.
Source: Own calculations are based on data from the German AFiD panel. Regional controls are from the statistical offices of the 16 German Laender.

ties)[11], ca. 11% large establishments ($\geq$ 250 employees or > €50 million turnover)[12], and 27% establishments belonging to multi-establishment firms. The share of corporations slightly increased from 60% in 1996 to 70% in 2016 (see Panel C of Figure B.2.1 in Appendix), while the share of large establishments and multi-establishments remained quite stable (see Panel A and Panel B of Figure B.2.1 in Appendix , respectively). The average establishment has a capital stock of €6 million and a turnover of €50 million. Regional controls at the county level indicate that the average GDP per capita is €29,225, the average unemployment rate is 8%, and the average population is 39,097.

[11]Note that we define establishments with another legal form as corporations.

[12]We define firm size classes according to the recommendation of the European Commission (2003/361/EC); available at `https://ec.europa.eu/growth/smes/sme-definition_en`).

We also provide summary statistics separately for small establishments (< 250 employees) (Panel A) and large establishments ($\geq$ 250 employees) (Panel B) in Table B.2.1 in Appendix . These statistics reveal that large establishments have a much larger probability of investing in buildings, while small establishments reside in smaller municipalities with a slightly lower GDP per capita.

2.3. Conceptual Framework

Many studies evaluate the effect of tax policy on firms' investment decisions through the tax term of the traditional user costs of capital (e.g. Hall and Jorgenson 1967; Devereux and Griffith 2003; Dwenger 2014). If we abstain from considering tax credits and tax benefits from debt finance (e.g., Cohen, Hansen, and Hassett 2002; Devereux and Griffith 2003; Giroud and Rauh 2019), taxes affect the pre-tax user costs of capital through the statutory tax rate (τ) and the present value of depreciation deductions (τD). The user cost of capital is the sum of the rate of economic depreciation (δ) and the after-tax cost of capital (ρ). It is generally assumed that firms choose their optimal level of investment by setting the marginal product of capital $f'(k)$ equal to the user cost of capital (Hall and Jorgenson 1967):

$$f'(k) = \frac{(\rho + \delta)(1 - \tau D)}{1 - \tau}. \tag{2.3}$$

The impact of the tax rate τ on the investment decision varies with the present value of depreciation deductions D. If D takes a value of one (immediate write-off in the investment period), the tax term takes a value of one and does not affect the user costs of capital (see also the literature on cash flow taxes; e.g., Auerbach and Bradford 2004). As D decreases for longer depreciation periods, the effect of the tax rate τ increases. Therefore, we expect that tax rate changes will have a stronger effect on investment goods without depreciation deductions (land)[13] or long depreciation periods (buildings) than goods with shorter depreciation periods (equipment).

A growing theoretical and empirical literature discusses how the lumpy nature of firm-level investment shapes the impact of business taxation on investment. Lumpy investment behavior suggests that firms either replace a considerable fraction of their existing capital (spikes) or do not invest at all (inaction). The literature (e.g., Bacchini et al. (2018) with further references) finds that different capital goods have heterogeneous investment dynamics and adjustment costs. Bontempi et al. (2004), for example, provide evidence of

[13]As land has an unlimited asset life, there is no depreciation deduction. Nevertheless, acquisition costs for land can be deducted in case of amortization or a land sale.

convex investment adjustment costs in the case of equipment and non-convex investment adjustment costs in the case of buildings. The lumpiness of investment arises from indivisibilities of investments, fixed costs of adjusting the capital stock (e.g., due to short-run capacity limits or production disruptions when investing), and partial irreversibility (e.g., Caballero and Engel 1999; Cooper and Haltiwanger 2006; Chen et al. 2022). As firms and establishments differ in relative distances from their adjustment thresholds, only a few establishments will adjust their capital investment in response to tax reforms, while a large fraction of establishments are likely to show inaction (Zwick and Mahon 2017). We thus expect to observe a stronger response of investments at the extensive margin than the intensive margin.

2.4. Identification Strategy

Our identification strategy relies on the common trends assumption between the (overlapping) treatment and control groups, that is, the average investment in treated and control establishments grow similarly in the absence of LBT rate changes. Our dependent variable, $I_{i,t}$, is either a dummy variable which equals 1 if the net investment of establishment i in year t is positive, and 0 otherwise (extensive margin) or the natural logarithm of net investment level of establishment i in year t (intensive margin).[14] Following Fuest, Peichl, and Siegloch (2018), we start our analysis by performing an event study design to demonstrate common pre-treatment trends and to identify different post-treatment adjustment periods:

$$I_{i,t} = \sum_{s=-4}^{5} \beta_s D_{i,t}^s + \alpha_i + Y_t + \epsilon_{i,t}, \tag{2.4}$$

where $D_{i,t}^s$ is a vector of dummy variables indicating a LBT rate change that occurs s years away, α_i denotes establishment fixed effects (FE), and Y_t year FE. We run separate regressions for LBT rate hikes, large LBT rate hikes, and LBT rate cuts. We define large LBT rate hikes as hikes exceeding the 75th percentile of the tax hike distribution. We set the event window from four years prior to five years after the LBT change, bin up dummy variables at the endpoint of the event window, and restrict our sample from 2001 to 2010 to ensure a sufficient number of years before and after LBT rate changes.

We interpret changes in LBT rates as exogenous shocks in a generalized DiD design. Establishments that are located in municipalities without a LBT change in a given year serve as a control group. By comparing investment responses of treated establishments with untreated establishments, we can mitigate the confounding effects of unobserved

[14] A log-level form accounts for nonlinearities and is more robust to measurement error compared to regressions with scaled investment as the dependent variable (Zwick and Mahon 2017).

variations in economic conditions and other spurious factors. As municipalities are much larger than establishments (89% of establishments have less than 250 employees), we consider their tax policy as exogenous from the perspective of an establishment. As Section 2.5.1 documents, we find a common trend for treated and untreated establishments. Concerning the potential endogeneity of tax rate changes to economic conditions, we show in Section 2.5.2.2 that LBT reforms are not driven by business cycles, which is also shown by Fuest, Peichl, and Siegloch (2018). We implement our estimation strategy by using the following generalized DiD regression:

$$I_{i,t} = \beta_0 + \beta_1 \tau_{i,t} + \gamma X_{i,t} + \delta R_{c,t} + \alpha_i + Y_t + \epsilon_{i,t} \tag{2.5}$$

where $I_{i,t}$ is either the net investment probability of establishment i in year t or the natural logarithm of the net investment level of establishment i in year t. The coefficient of interest, β_1, measures the change in the investment probability in percentage points or the investment level in percent induced by a one percentage point increase in τ.

$X_{i,t}$ is a vector of control variables at the establishment level, including the logarithm of the capital stock to account for investment opportunities and the logarithm of turnover to control for the establishment size. As capital stocks are not recorded in our establishment data, we use the information on depreciation expenses from the Cost Structure Survey, the investment mix from the Investment Survey, and average depreciation periods from the German tax code to calculate the establishment-level capital stocks (see Wagner 2010; Eichfelder, Hechtner, and Hundsdoerfer 2018, and Appendix for further explanations). The Cost Structure Survey also adds information about the legal form. $R_{c,t}$ is a vector of county-level control variables that account for heterogeneity in economic conditions across counties, including the logarithm of GDP per capita, unemployment rate, and the logarithm of population.

The establishment FE, α_i, capture unobserved but fixed differences between establishments. We add year FE, Y_t, to control for common economic shocks and trends (e.g., business cycle). We allow for correlation between standard errors across time and establishments within municipalities by clustering standard errors at the municipality level, which is the level of our identifying variation. As a robustness test, we cluster standard errors at the firm level, because investment decisions are made by the firm rather than the establishment itself.

Given that about 27% of the establishments in our panel belong to multi-establishment firms, we also estimate the share of the response due to capital reallocation from high- to low-tax establishments.

2.5. Effect of Local Business Tax Rates on Investment

2.5.1. Baseline Results

We start by discussing the results from the event study model of Equation (2.4). Figure 2.3 illustrates the estimates and corresponding 95% confidence intervals of our event study models when we use the net investment probability (Panel A) and the net investment level (Panel B) as the dependent variables. The detailed regression results are reported by Tables C.2.1 and C.2.2 in Appendix . We observe smooth pre- and post-trends for LBT rate hikes, validating common trends of investment activity for both groups before and after LBT rate hikes and large LBT rate hikes that are by far the majority of our tax rate variations (9,839 hikes vs 863 cuts).

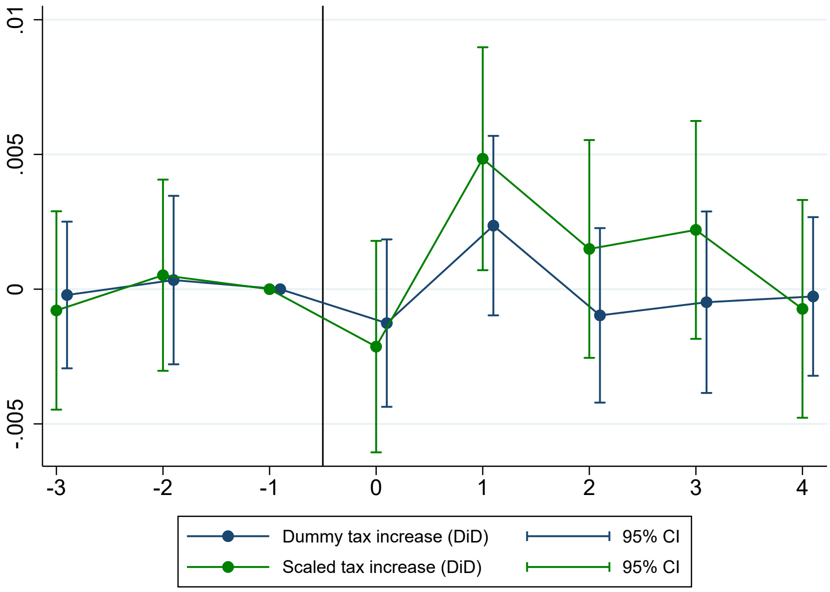
For LBT rate cuts, coefficients and precision vary. In Panel B of Figure 2.3, the coefficients are significantly positive in t and $t + 2$ of the LBT rate cut. Therefore, we perform an additional check (see Tables 2.4 and 2.5, column (6)), where we exclude establishments subject to LBT rate cuts. Taken together, graphical evidence suggests common trends of treated and untreated establishments with only a weak investment response to LBT rate changes.

Next, we present and discuss the results of the generalized DiD regression of Equation (2.5). Table 2.4 shows the effect of LBT rates on the net investment probability, that is, the extensive margin. Table 2.5 shows the effect of LBT rates on the net investment level, that is, the intensive margin. We estimate a multitude of models to test whether our parameter estimates are sensitive to specification choices: we add establishment controls in column (2), regional controls in column (3), cluster standard errors at the firm level in column (4), and include industry-year FE to account for industry-wide shocks in column (5).[15] In column (6), we exclude observations for LBT rate cuts from our sample. Business investment may not respond immediately to changes in the after-tax user cost of capital because adjusting the capital stock is costly. To account for a time lag in the LBT rate effect on investment, we use the first lag of the LBT rate as the explanatory variable in column (7). In column (8), we use the net investment and the wage-weighted LBT rate at the firm level to take formula apportionment between establishments into account.

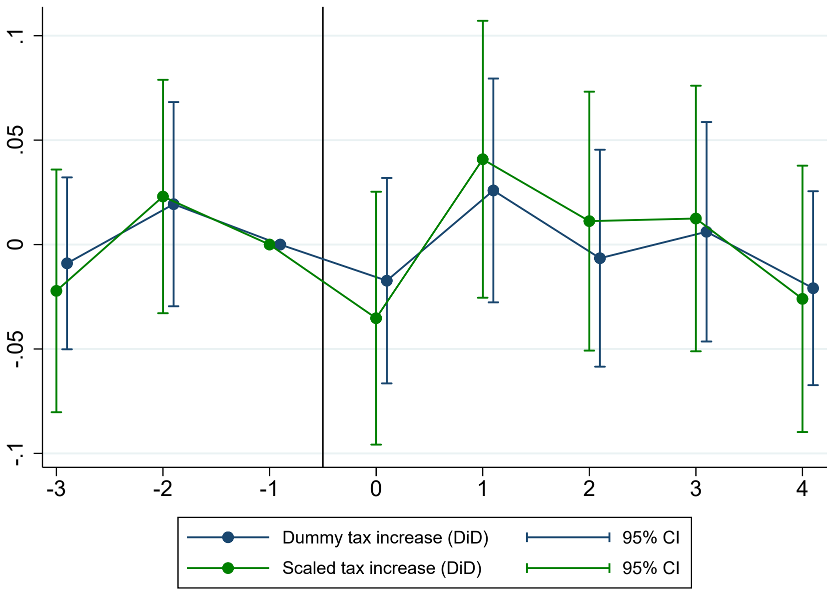
[15] We construct the industry index using the classification of industry sectors (*Klassifikation der Wirtschaftszweige*), version 1993 for the years 1995–2002, version 2003 for the years 2003–2007, and version 2008 for the years 2008–2016. In sum, we distinguish between 15 sectors in the manufacturing industry.

Figure 2.3: Event study model

a Net investment probability



b Net investment level



Note: This figure illustrates the estimates and corresponding 95% confidence intervals of the different event study models (i.e., LBT rate hike, large LBT rate hike, and LBT rate cut) for the net investment probability (Panel A) and the net investment level (Panel B). We define large LBT rate hikes as hikes exceeding the 75th percentile of the tax hike distribution. Tables C.2.1 and C.2.2 in Appendix report the corresponding estimates.

Table 2.4: Effect of LBT rates on net investment probability (extensive margin)

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
LBT rate	-0.114	-0.084	-0.064	-0.064	-0.057	-0.060		
	(0.107)	(0.104)	(0.103)	(0.0960)	(0.103)	(0.109)		
LBT rate$_{t\text{-}1}$							-0.050	
							(0.115)	
Adj. LBT rate								0.089
								(0.073)
Capital stock		0.027***	0.027***	0.027***	0.027***	0.026***		0.038***
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)		(0.001)
Capital stock$_{t\text{-}2}$							0.008***	
							(0.001)	
Turnover		0.048***	0.048***	0.048***	0.047***	0.049***		0.012***
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)		(0.002)
Turnover$_{t\text{-}2}$							0.025***	
							(0.002)	
Constant	0.927***	-0.225***	-0.0828	-0.083	-0.018	-0.070	0.533***	0.186***
	(0.016)	(0.030)	(0.053)	(0.052)	(0.055)	(0.056)	(0.055)	(0.037)
Regional controls			✓	✓	✓	✓	✓	✓
Establishment FE	✓	✓	✓	✓	✓	✓	✓	
Firm FE								✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Industry-year FE					✓			
Observations	817,372	817,372	817,372	817,372	817,372	763,736	693,060	358,144
Adjusted R-squared	0.377	0.389	0.389	0.389	0.390	0.392	0.390	0.541

Note: The dependent variable is the net investment probability of establishment i in year t. Regional controls comprise the logarithm of GDP per capita, the unemployment rate, and the logarithm of population. We distinguish 15 sectors in the manufacturing industry in column (5). We exclude observations with LBT rate cuts in column (6). We use the first lag of the LBT rate and second lags of the establishment controls in column (7). We use the net investment probability and the wage-weighted LBT rate at the firm level in column (8). Estimations are performed by ordinary least squares (OLS). Standard errors (in parentheses) are clustered at the municipality level or at the firm level in column (4). ***, *** and * indicate significance levels of 0.01, 0.05 and 0.1, respectively.

Table 2.5: Effect of LBT rates on net investment level (intensive margin)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
LBT rate	-0.786	-0.648	-0.443	-0.443	-0.436	-0.965*		
	(0.577)	(0.515)	(0.512)	(0.491)	(0.513)	(0.542)		
LBT rate$_{t-1}$							0.022	
							(0.584)	
Adj. LBT rate								-0.203
								(0.452)
Capital stock		0.093***	0.093***	0.093***	0.095***	0.083***		-0.028***
		(0.005)	(0.005)	(0.005)	(0.005)	(0.005)		(0.009)
Capital stock$_{t-2}$							0.025***	
							(0.003)	
Turnover		0.633***	0.636***	0.636***	0.617***	0.629***		0.986***
		(0.012)	(0.012)	(0.012)	(0.012)	(0.012)		(0.015)
Turnover$_{t-2}$							0.318***	
							(0.009)	
Constant	12.54***	1.010***	1.361***	1.361***	1.919***	1.961***	7.301***	-3.013***
	(0.088)	(0.196)	(0.295)	(0.300)	(0.301)	(0.310)	(0.298)	(0.254)
Regional controls			✓	✓	✓	✓	✓	✓
Establishment FE	✓	✓	✓	✓	✓	✓	✓	
Firm FE								✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Industry-year FE					✓			
Observations	710,747	710,747	710,747	710,747	710,747	663,538	606,380	332,226
Adjusted R-squared	0.634	0.647	0.647	0.647	0.648	0.648	0.651	0.817

Note: The dependent variable is the net investment level of establishment i in year t. Regional controls comprise the logarithm of GDP per capita, the unemployment rate, and the logarithm of population. We distinguish 15 sectors in the manufacturing industry in column (5). We exclude observations with LBT rate cuts in column (6). We use the first lag of the LBT rate and second lags of the establishment controls in column (7). We use the net investment level and the wage-weighted LBT rate at the firm level in column (8). Estimations are performed by ordinary least squares (OLS). Standard errors (in parentheses) are clustered at the municipality level or at the firm level in column (4). ***, ** and * indicate significance levels of 0.01, 0.05 and 0.10, respectively.

Across all specifications, the effect of LBT rates on the net investment probability (Table 2.4) and on the net investment level (Table 2.5) is not statistically different from zero, except for the LBT rate at the intensive margin in column (6) of Table 2.5. The parameter estimates are negative in almost all specifications. Our coefficients suggest that an increase of the LBT rate by one percentage point decreases the net investment probability by 0.05 percentage points to 0.11 percentage points and the net investment by 0.4% to 0.9%. With an sample average investment probability of 87%, this implies a semi-elasticity for the net investment probability of -0.05 to -0.13 and the net investment level of -0.4 to -1.03. Given that the standard errors at the intensive margin are about 0.5%, we cannot reject the null hypothesis of a semi-elasticity below -1. However, our finding on the small and statistically nonsignificant average responsiveness might conceal very different responses across investment and firm types, which we investigate in Section 2.5.3.

The positive and statistically significant coefficients for our establishment controls confirm our expectations that investment activity increases with size and investment opportunities. In column (7), we use second lags for establishment controls to account for potential anticipation effects of LBT reforms.

2.5.2. Robustness

2.5.2.1. German Business Tax Reform 2008

The GBTR 2008 created heterogeneous effects across legal forms that largely increased the effective LBT burden of corporations compared to owners of pass-through entities (see Section 2.2.1). Consequently, corporate establishments whose shareholders cannot credit LBT payments against their PIT should be more responsive to LBT variation than pass-through entities. To test this empirically, we extent our baseline regression Equation (2.5) to a triple-difference regression:

$$\begin{aligned} I_{i,t} = &\beta_0 + \beta_1 \tau_{i,t} + \beta_2 Corp_{i,t} + \beta_3 Reform08_t \\ &+ \beta_4 (\tau_{i,t} \cdot Corp_{i,t}) + \beta_5 (\tau_{i,t} \cdot Reform08_t) + \beta_6 (Corp_{i,t} \cdot Reform08_t) \\ &+ \beta_7 (\tau_{i,t} \cdot Corp_{i,t} \cdot Reform08_t) \\ &+ \gamma X_{i,t} + \delta R_{c,t} + \alpha_i + Y_t + \epsilon_{i,t}, \end{aligned} \tag{2.6}$$

where $Corp_{i,t}$ is an indicator variable that equals 1 if an establishment belongs to a corporation, and 0 otherwise; $Reform08_t$ is a dummy variable that equals 1 for years after the reform in 2008, and 0 otherwise. The triple-difference interaction term between $\tau_{i,t}$, $Corp_{i,t}$, and $Reform08_t$ captures the investment difference between treated and non-treated establishments (*first difference*), across corporations and pass-through entities (*second difference*), and before and after the GBTR 2008 (*third difference*).

We report the regression results in Table C.2.3 in Appendix using the same specifications (controls, fixed effects, standard errors) as in our baseline model. The number of observations is lower as the information on the legal form is only available for about 44.3% of the sample. The coefficients of the triple-difference interaction term are typically negative but not statistically different from zero in any specification. Although the GBTR 2008 largely reduced the effective LBT burden on pass-through entities compared to corporations, we cannot provide statistically significant evidence that changes in LBT rates had a stronger effect on corporations relative to pass-through entities.

This result confirms our baseline findings. Again, we obtain negative coefficients for tax rates, which are not significantly different from zero. A potential explanation is heterogeneity in responses, which produces a negative average estimate but is imprecisely estimated. We analyze heterogeneity in investment responses in more detail in Section 2.5.3.

2.5.2.2. Business-Cycle Shocks

If local business-cycle shocks trigger LBT reforms, our estimates on the LBT rate could be biased. We use the event study model in Equation (2.4), formalized in Section 2.4, to address the potentially confounding effects of time-varying economic conditions across municipalities. We report the results in our Appendix . Figure C.2.1 illustrates the estimates and corresponding 95% confidence intervals from regressing the natural logarithm of GDP per capita and the unemployment rate, both at the county level on a set of dummy variables which indicate LBT reforms. More detailed regression results are provided by Tables C.2.4 and C.2.5. Panel A of Figure C.2.1 shows smooth (pre-)trends for (large) LBT rate hikes and cuts, validating that changes in local economic conditions measured by GDP per capita are not correlated with (large) LBT hikes and cuts. The estimates for LBT rate cuts in Panel B of Figure C.2.1 are rather inconclusive but mostly statistically nonsignificant. Concluding, this evidence helps us to rule out that structural economic differences between municipalities are drivers of LBT reforms.

2.5.2.3. Additional Tests

In Table C.2.6 in Appendix , we report regression results for alternative dependent variables that also consider investments in leased assets. Similar to our baseline results, we do not find statistically significant investment responses to tax rate changes in these models. In Table C.2.7 in the Appendix, we also estimated models with two-way fixed effects that account for continuous treatments by using the estimator proposed by De Chaisemartin and d'Haultfoeuille (2020). These alternative specifications do not lead to different implications.

2.5.3. Heterogeneity in Investment Responses

2.5.3.1. Investment Goods with Different Depreciation Length

As pointed out in Section 3.3, we expect larger responses for investment goods with long-depreciation periods. Our data allows us to differentiate between investments in land, buildings, and equipment, so that we can disentangle the effect of LBT rates on investment goods with different depreciation periods and deductions. Table 2.6 reports the results from estimating the baseline regression Equation (2.5) separately for the three capital goods. Columns (1) and (2) refer to the land investment probability and level, columns (3) and (4) to the building investment probability and level, and columns (5) and (6) to the equipment investment probability and level, respectively.

Table 2.6: Heterogeneity tests: Types of investment goods

	Land	**Land**	**Build.**	**Build.**	**Equip.**	**Equip.**
	Prob.	*Level*	*Prob.*	*Level*	*Prob.*	*Level*
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
LBT rate	-0.091*	4.824	-0.621***	-0.106	-0.077	-0.204
	(0.052)	(6.079)	(0.127)	(1.485)	(0.107)	(0.499)
Establishment controls	✓	✓	✓	✓	✓	✓
Regional controls	✓	✓	✓	✓	✓	✓
Establishment FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Observations	817,372	24,933	817,372	156,646	817,372	707,559
Adjusted R-squared	0.157	0.159	0.354	0.281	0.384	0.663

Note: The dependent variables are the logarithm of land investment probability of establishment i in year t in column (1), the land investment level of establishment i in year t in column (2), the logarithm of building investment probability of establishment i in year t in column (3), the building investment level of establishment i in year t in column (4), the logarithm of equipment investment probability of establishment i in year t in column (5) and the equipment investment level of establishment i in year t in column (6). Establishment controls comprise the logarithm of the capital stock and the logarithm of turnover. Regional controls comprise the logarithm of GDP per capita, the unemployment rate, and the logarithm of population. Estimations are performed by ordinary least squares (OLS). Standard errors (in parentheses) are clustered at the municipality level. ***, ** and * indicate significance levels of 0.01, 0.05 and 0.1, respectively.

The impact of LBT rate changes on the investment types is negative throughout, except for land at the intensive margin.[16] Yet, only the probability of investing in land or buildings is significantly affected by tax rate changes, confirming our expectations in two ways: First, we only find a significant response for long-term investment goods with *small depreciation deductions* (land and buildings). Second, we only find a significant response for *lumpy investments* (response at the extensive margin).

The regression coefficients suggest that a change in the LBT rate by one percent-

[16]Note that the number of observations for investments in land at the intensive margin is very small because the probability to invest in land is only 3%. Due to the limited number of observations with positive land investments in our sample, this specification should be interpreted with caution.

age point reduces the probability of investing in land by 0.09 percentage points and the probability of investing in buildings by 0.62 percentage points. If we further consider the sample average probabilities of investing in land (3%) and buildings (19%), this implies semi-elasticities of investment at the extensive margin of -3.02 for land and -3.26 for buildings. In other words, a one percentage point increase in the LBT rate reduces the probability of investing in land (buildings) by 3.02% (3.26%). Thus, we document economically large investment responses at the extensive margin for land and building investments. The effect size is much larger than our baseline estimate in column (1) of Table 2.4. Consistent with the prior literature (e.g., Bond and Xing 2015; Zwick and Mahon 2017; Eichfelder and Schneider 2018), our findings suggest that capital goods with longer depreciation periods are more responsive to tax incentives, such as bonus depreciation or tax credits, and tax rate changes. Note that results also hold for (unreported) alternative control settings and alternative clustering approaches of standard errors.

2.5.3.2. Business Characteristics

Table 2.7 reports results from estimating the baseline regression Equation (2.5) using the net investment probability as the dependent variable (extensive margin) and interacting the LBT rate with a set of business characteristic dummies. Table C.2.8 in Appendix shows the respective results for the intensive margin.

We expect that large establishments respond more strongly, because the costs of complying with the tax law exhibit substantial economies of scale.[17] Consistent with this prediction, columns (1) and (2) show that a one percentage point LBT rate increase additionally reduces the probability of investing for large establishments ($\geq$ 250 employees or $>$ €50 million turnover, respectively) by about 0.2 to 0.3 percentage points.[18]

High-productivity establishments might respond more strongly, because the burden of taxation increases with taxable profits, which is positively associated with productivity (e.g., Devereux and Griffith 2003). Tax rates become largely irrelevant if establishments have taxable profits below zero. While deductions from the tax base, such as depreciation allowances, are thus more relevant for the tax burden of low-productivity establishments,

[17]Compared to sales revenue or the number of employees, the burden of dealing with the tax law for small enterprises can be more than ten times as large as the corresponding burden for large firms. Resulting from quasi-fixed costs, better tax knowledge (e.g., internal tax staff, more sophisticated tax advisers), and technological advantages (software), larger firms are more cost-efficient in their tax affairs (Eichfelder and Vaillancourt 2014 with further references). Eichfelder and Schneider (2018) find a stronger investment response of large firms to a German bonus depreciation regime, and Knittel (2007) and Kitchen and Knittel (2011) find higher take-up rates of large firms for bonus depreciation in the US.

[18]For the US bonus depreciation regime, Zwick and Mahon (2017) find a larger investment response for smaller firms. They explain their finding with larger liquidity benefits as these firms are typically capital-constrained. In our view, this does not contradict our analysis, as our small establishments are significantly smaller than the small firms in the sample of Zwick and Mahon (2017). Thus, the costs of dealing with tax complexity should be more important in our setting.

Table 2.7: Heterogeneity tests: Business characteristics (extensive margin)

	(1)	(2)	(3)	(4).	(5)	(6)
LBT rate	-0.022	-0.033	0.132	-0.035	-0.016	0.027
	(0.105)	(0.106)	(0.127)	(0.105)	(0.146)	(0.107)
LBT rate · Large estab. (empl.)	-0.332***					
	(0.079)					
LBT rate · Large estab. (turn.)		-0.219***				
		(0.072)				
LBT rate · High-prod. estab. (median)			-0.354***			
			(0.095)			
LBT rate · High-prod. estab. (top 10%)				-0.262*		
				(0.138)		
LBT rate · Corporation					0.029	
					(0.095)	
LBT rate · Estab. of multi-estab. firm						-0.298***
						(0.080)
Establishment controls	✓	✓	✓	✓	✓	✓
Regional controls	✓	✓	✓	✓	✓	✓
Establishment FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Observations	817,372	817,372	817,372	817,372	362,441	817,372
Adj. R-squared	0.389	0.389	0.389	0.389	0.463	0.389

Note: The dependent variable is the investment probability of establishment i in year t. Establishment controls comprise the logarithm of the capital stock and the logarithm of turnover. Regional controls comprise the logarithm of GDP per capita, the unemployment rate, and the logarithm of population. Productivity is the ratio of gross profit (turnover less wage expenses) over total number of employees. Estimations are performed by ordinary least squares (OLS). Standard errors (in parentheses) are clustered at the municipality level. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

tax rates matter more for firms with high productivity and taxable profits. We measure productivity as the ratio of gross profit, defined as turnover less wage expenses over the total number of employees.[19] Columns (3) and (4) show that a one percentage point LBT rate increase additionally reduces the probability of investing for high-productivity establishments by about 0.3 to 0.4 percentage points.

Similar to our analysis of the GBTR 2008 in Section 2.5.2.1, we test if corporations behave differently from pass-through entities that had access to LBT tax credits under German tax law. In this specification, we rely on 362,441 observations as information on the legal form is only available for 44.3% of the sample. In line with the results in Section 2.5.2.1, we do not find statistically different investment responses for corporations compared to pass-through entities in column (5). Note that corporations are not more productive than pass-through entities, as Panel D of Figure B.2.1 in Appendix shows

[19] As earnings are not available at the establishment level, we proxy profits by the difference of turnover and wage expenses. We scale this variable by the number of employees to obtain a measure of productivity.

that the share of corporations is stable across productivity quintiles.

As multi-establishment firms are able to reallocate their investment activity to establishments in low-tax municipalities (e.g., Giroud and Rauh 2019), establishments belonging to multi-establishment firms might respond more strongly. This argument is in line with economic evidence that benefits from tax planning may affect real investment activity of firms (Overesch 2009). Column (6) shows that a one percentage point increase in the LBT rate additionally reduces the probability of investing for multi-establishments by about 0.3 percentage points.

Our analysis of business characteristics indicates that larger establishments, highly productive establishments, and establishments of multi-establishment firms react stronger than their counterparts at the extensive margin. Our findings are consistent with the argument that resources matter for tax planning and considering taxes in investment decisions. Therefore, firm units with lower planning costs (large establishments, multi-establishment firms), more tax avoidance opportunities (multi-establishment firms), and higher burdens of tax rates (high-productivity establishments) are more responsive to changes in tax rates in their investment decisions.

2.5.3.3. Reallocation of Investment

As discussed in Section 2.5.3.2, multi-establishment firms can reallocate real capital inputs to establishments in low-tax municipalities in case of LBT rate changes.[20] Giroud and Rauh (2019) estimate investment elasticities to US state tax changes, with about half of the effect being driven by the reallocation of real activity (establishments and employees) to low-tax states.

To identify the reallocation of real capital between different establishments of the same firm, we calculate the variable, $LBT\ rate\ differential_{i,t}$, as a new explanatory variable and re-estimate our baseline regression Equation (2.5) for multi-establishment firms only. The variable, $LBT\ rate\ differential_{i,t}$, is the difference between establishment i's LBT rate in municipality j ($\tau_{j,t}$) and the unweighted average LBT rate over all other establishments of the multi-establishment firm in other municipalities ($\tilde{\tau}_{-j,t}$).

We report our regression results in Table 2.8. Similar to Eichfelder, Hechtner, and Hundsdoerfer (2018), we run a subsample analysis for multi-establishment firms with a low number of establishments (2-3 establishments) and firms with more than four establishments. In line with Eichfelder, Hechtner, and Hundsdoerfer (2018), we find stronger investment responses for groups with a low number of establishments. A potential ex-

[20] Formula apportionment should mitigate profit shifting opportunities among municipalities of multi-establishment firms. There exists some evidence that multi-establishment firms still engage in tax avoidance under formula apportionment by manipulating wage expenses (e.g., Riedel 2010; Eichfelder, Hechtner, and Hundsdoerfer 2018).

Table 2.8: Heterogeneity tests: Reallocation of investments for multi-establishment firms

	Total	**Total**	**2-3 estab.**	**2-3 estab.**	**4+ estab.**	**4+ estab.**
	Prob.	*Level*	*Prob.*	*Level*	*Prob.*	*Level*
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
LBT rate differential	-0.108	-1.282*	-0.118	-1.855**	-0.187	0.001
	(0.135)	(0.736)	(0.170)	(0.882)	(0.275)	(1.605)
Establishment controls	✓	✓	✓	✓	✓	✓
Regional controls	✓	✓	✓	✓	✓	✓
Establishment FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Observations	170,012	145,940	101,186	87,674	68,826	58,266
Adjusted R-squared	0.403	0.752	0.442	0.723	0.373	0.785

Note: The dependent variables are the logarithm of net investment probability of establishment i in year t in columns (1), (3), and (5) and the net investment level of establishment i in year t in columns (2), (4), and (6). Establishment controls comprise the logarithm of the capital stock and the logarithm of turnover. Regional controls comprise the logarithm of GDP per capita, the unemployment rate, and the logarithm of population. Estimations are performed by ordinary least squares (OLS). Standard errors (in parentheses) are clustered at the municipality level. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

planation for this finding is that investments adjustment costs, such as costs of reallocating investments, increase in the number of establishments. An alternative explanation is that the identification of tax incentives becomes more challenging as the number of establishments increases. In columns (2) and (4), the coefficients of the *LBT rate differential*$_{i,t}$ are negative and statistically significant. This underlines our argument that multi-establishment firms might adjust their investment portfolios and their allocation of production factors in a formula apportionment regime to save taxes. Interestingly, we only observe significant effects for investments at the intensive margin in these specifications. This could be interpreted as evidence that in a multi-establishment context, investments are less lumpy than in a single-establishment context.

2.6. Conclusion

We investigate the effect of LBT rate changes on real business investment at the extensive and intensive margin using administrative survey data of manufacturing establishments in Germany. This paper provides solutions for three major methodological problems for identifying the causal effect of business tax rates on investment decisions.

First, by applying a generalized DiD approach with establishments as the most granular observation unit, we are able to use 10,702 LBT rate changes from 1995 to 2016 for identification. Second, as accounting data or data on FDI stocks and flows used in prior studies can be affected by financial investments and tax avoidance, we rely on survey data provided by the German federal statistical office for investments in land, buildings, and equipment. Using such mandatory information also addresses concerns regarding self-selection and the representative nature of accounting data. Third, by focusing on the German LBT system in which municipalities are only allowed to choose a specific tax multiplier while the tax base is set solely at the federal level, we are able to disentangle the effect of tax rate changes from tax base changes.

In our baseline results, we do not find statistically significant evidence that exogenous variations in LBT rates affect the business investment of German manufacturing establishments at extensive or intensive margin. Considering the consistently negative regression coefficients and high standard errors, this should be due to the heterogeneity in investment responses. Taking account of investment type and firm heterogeneity reveals that LBT rates exert a significant impact on the probability of investments (extensive margin) (a) with long depreciation periods (land and buildings), (b) of large establishments ($\geq$ 250 employees or $>$ €50 million turnover), (c) of high-productivity establishments, and (d) of establishments belonging to multi-establishment firms.

It should be noted that this study is subject to some limitations, while the central area of concern is the external validity of our findings. We rely on a sample of manufacturing establishments in Germany that might differ from other industries and countries. In addition, investment choices in response to local business taxes might differ from investment decisions considering international taxation. This might be an additional issue of investment heterogeneity as international investments are typically performed by international firms that are active in more than one country.

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Appendices

Appendix A: Calculation of the Capital Stock

Our calculation is based on Wagner (2010), who uses depreciation values for tax purposes reported in the Cost Structure Survey, information on the composition of investments from the Investment Survey and average depreciation periods for different asset classes (buildings and equipment) to compute capital stocks. Our method extends this approach in a number of ways and can be described by

$$K_{i,t-1} = \left(D_{it} \cdot \left(\alpha_{it}^{E} \cdot P_{t}^{E} + \alpha_{it}^{B} \cdot P_{t}^{B}\right) - I_{it}^{N}\right) \cdot \frac{1}{2}, \qquad \text{(A.2.1)}$$

where $K_{i,t-1}$ is the capital stock at the end of the previous period (or beginning of the current period) of the firm i, D_{it} is the depreciation of i in t, α_{it}^{E} is the fraction of equipment investment of a given year, α_{it}^{B} the fraction of building investment in that year, and P_{t}^{E} $\left(P_{t}^{B}\right)$ the average depreciation period for equipment (building) investment in Germany in t.

Multiplying the sum of depreciation with the average depreciation period yields the investment value at the beginning of the operating period. To account for depreciation after the beginning of the operating period of an asset, we divide this value by two. Therefore, we assume that the average operating period has expired by a factor of 50% for each asset. This implies further that price-adjusted depreciations are approximately evenly distributed over time. Note that investments in t have a positive effect on D_{it}. If investments are executed in the middle of the year, D_{it} should rather be a measure of the capital stock in the middle of the period instead of the beginning of the period. To account for that aspect, we deduct 50% of net investments I_{it}^{N} (defined as gross investment minus disinvestment) of firm i in time t.

The depreciation period P_{t}^{E} for equipment is assumed to be 7 years (see Devereux et al. 2009). For new buildings, the regular periods are 25 years (for old buildings 40 to 50 years). For simplicity, we do not account for declining depreciation schemes for buildings. This can be justified by the fact that declining schemes increase the present values of depreciation allowances but not the average depreciation over the depreciation period. The composition of different asset classes is estimated by the distribution of investments α_{it}^{E} and α_{it}^{B} of the manufacturing industry in our data, with $\alpha_{it}^{E} + \alpha_{it}^{B} = 1$. To account for measurement error, we calculate average values for α_{it}^{E} and α_{it}^{B} by year, industry, business size (large firms compared to small firms with up to 250 staff members) and region (East versus West Germany).

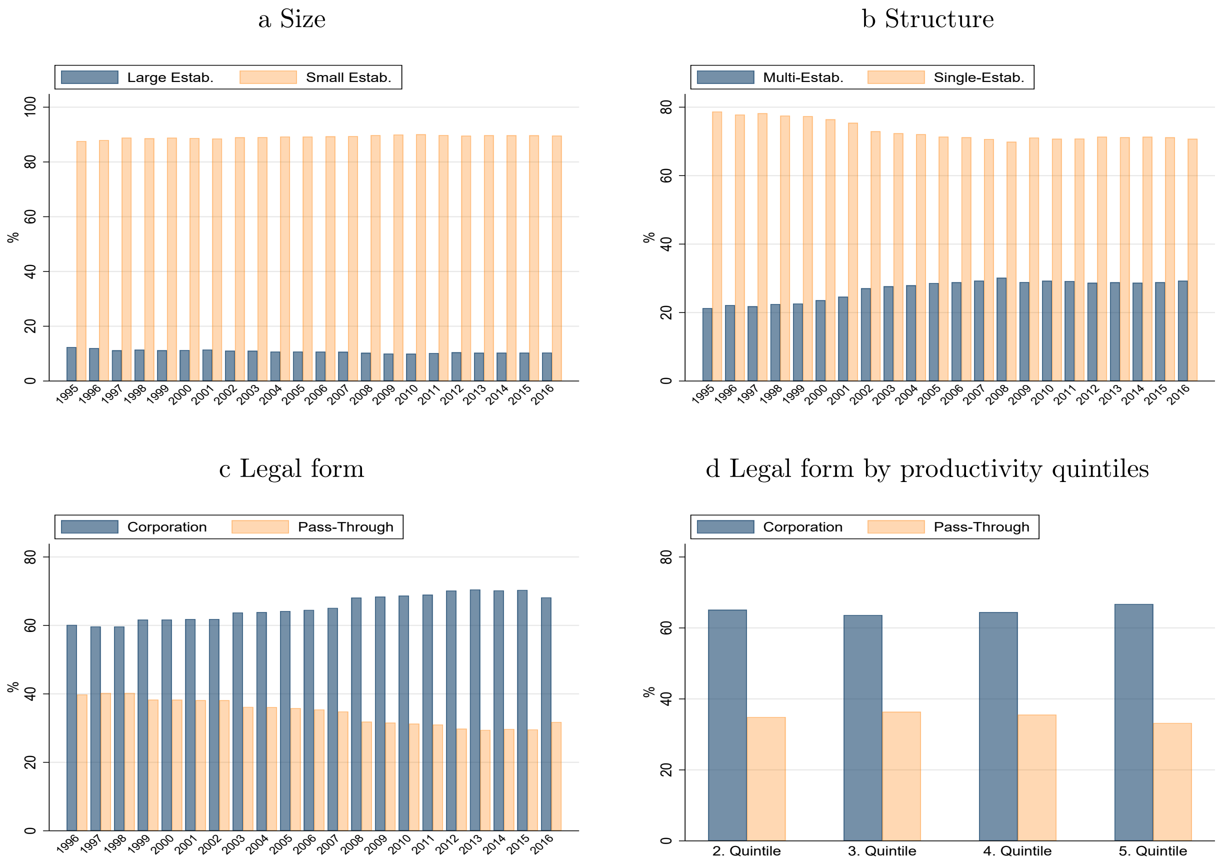
The tax depreciation period for new buildings increased to 33.3 years in 2001, while depreciation periods for modernization remained unchanged. The increased depreciation period is only relevant for new installments. Thus, considering economic growth and declining depreciation schemes of preceding periods, we assume a declining adaptation process of the average depreciation period per firm over 25 years with $D_{2000+x} = D_{2000} + \Delta \cdot \sqrt{\frac{x}{25}}$, where D_{2000} denotes the average depreciation period in 2000 (29 years on average for old and new buildings), x the number of years after 2000 and the increase in the average depreciation period resulting from the reform. This yields an average depreciation period for buildings of 35.66 years in 2008.

The computation of capital stock may be affected by measurement error in D_t. This is especially a problem for a high variation of tax depreciation over time, implying a fluctuating capital stock. To account for that, we rely on estimated capital stocks of future periods to obtain a more consistent estimate of the capital stock of preceding periods. Hence, we define the capital stock of the preceding period as the capital stock of the following period plus investments and minus depreciation and disinvestments in t. In addition to fixed assets and extending Wagner (2010), we consider leased investments as increasing the effective capital-in-kind. We rely on data from the Investment Survey to compute the ratio of leased assets to fixed assets by year, industry, business size and region (West versus East Germany). The value of fixed assets of each firm is multiplied by one plus the computed ratio.

A drawback of our data is that depreciation volumes of the Cost Structure Survey are only available at the firm level. Therefore, we allocate depreciation to the establishment. We compute the ratio of the capital stock to the number of staff members by year, industry, business size (large firms compared to small firms with up to 250 staff members) and region (establishments in the West and establishments in the East). Using these ratios, we allocate the firms' capital stock to the establishments.

Appendix B: Descriptive Statistics

Figure B.2.1: Characteristics of the main analysis sample, 1995-2016



Note: This figure illustrates the composition of our establishment-year observations by size (N = 817,372), establishment structure (N = 817,372), legal form (N = 362,441), and legal form by productivity quintiles (N = 362,441). The visualization of 1995 in Panel C and the first quintile in Panel D is impossible due to confidentiality restrictions.
Source: Own calculations based on data from the German AFiD panel.

Figure B.2.2: Investment probability, 1995-2016

Note: This figure illustrates the share of establishments not investing in buildings or equipment (none), investing in both buildings and equipment, investing in buildings only and investing in equipment only. Note that the mean value of the share of establishments investing in buildings is only 0.34%.
Source: Own calculations based on data from the German AFiD panel.

Table B.2.1: Summary statistics for the main analysis sample by size, 1995–2016

Panel A: Small estab. (< 250 empl.)	**N**	**Mean**	**Std. dev.**	**Percentiles**		
				25th	**50th**	**75th**
Investment variables						
Gross investment (1,000 €)	729,421	426	1,630	16	97	356
Gross investment probability (%)	729,421	86	35	100	100	100
Equipment investment (1,000 €)	729,421	358	1,410	14	89	312
Equipment investment probability (%)	729,421	85	35	100	100	100
Building investment (1,000 €)	729,421	63	477	0	0	0
Building investment probability (%)	729,421	15	36	0	0	0
Net investment (1,000 €)	729,421	413	1,619	14	91	339
Net investment probability (%)	729,421	86	35	100	100	100
Local business tax						
τ (%)	729,421	15	2	13	14	16
Establishment controls						
Capital stock (1,000 €)	729,421	2,284	3,751	741	1,324	2,593
Turnover (1,000,000 €)	729,421	13	44	3	6	14
Regional controls						
GDP per capita (€)	729,421	28,937	12,129	22,005	26,989	32,706
Unemployment rate (%)	729,421	8	4	6	8	10
Population	729,421	39,167	116,921	143	259	492

continues on next page

Table B.4.1: Summary statistics for the main analysis sample by size, 1995–2016 (continued)

Panel B: Large estab. ($\geq$ 250 empl.)	**N**	**Mean**	**Std. dev.**	**Percentiles**		
				25th	**50th**	**75th**
Investment variables						
Gross investment (1,000 €)	87,946	8,611	41,081	995	2498	6197
Gross investment probability (%)	87,946	96	19	100	100	100
Equipment investment (1,000 €)	87,946	7698	37440	888	2,203	5,395
Equipment investment probability (%)	87,946	96	19	100	100	100
Building investment (1,000 €)	87,946	865	5,458	0	100	272
Building investment probability (%)	87,946	50	50	0	100	100
Net investment (1,000 €)	87,946	8,523	41,043	947	2,425	6,089
Net investment probability (%)	87,946	96	19	100	100	100
Local business tax						
τ (%)	87,946	15	2	13	13	15
Establishment controls						
Capital stock (1,000 €)	87,946	34,901	129,736	8,253	14,425	27,228
Turnover (1,000,000 €)	87,946	256	1,222	55	94	183
Regional controls						
GDP per capita (€)	87,946	28,229	12,274	2,0467	24,818	32,789
Unemployment rate (%)	87,946	8	3	6	8	10
Population	87,946	38,512	123,004	145	269	505

Note: Net investment is gross investment less leased assets and capital sales. Investment probabilities capture the percentage of establishments with positive investments in percentage points. Investment quantities capture the investment per establishment in €1,000.
Source: Own calculations based on data from the German AFiD panel. Regional controls are from the Statistical Offices of the 16 German Laender.

Appendix C: Additional Results

Table C.2.1: Event study model: Net investment probability

	LBT hike	*Large LBT hike*	*LBT cut*
	(1)	**(2)**	**(3)**
F4	-0.00368	-0.00145	0.00942
	(0.00301)	(0.00594)	(0.0124)
F3	0.00136	0.00503	-0.00143
	(0.00322)	(0.00678)	(0.0136)
F2	-0.00311	-0.00430	0.0152
	(0.00308)	(0.00625)	(0.0145)
L0	-0.00191	-0.00641	0.0237
	(0.00345)	(0.00794)	(0.0151)
L1	-0.00357	-0.00272	-0.00737
	(0.00388)	(0.00786)	(0.0102)
L2	-0.000889	0.00338	-0.00489
	(0.00395)	(0.00833)	(0.0141)
L3	-0.00386	0.00151	-0.0219
	(0.00390)	(0.00861)	(0.0166)
L4	-0.00721*	0.00426	-0.00936
	(0.00433)	(0.00870)	(0.0133)
L5	-0.00612*	0.00261	-0.0131
	(0.00335)	(0.00795)	(0.00931)
Constant	0.903***	0.895***	0.905***
	(0.00633)	(0.00303)	(0.00517)
Establishment FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	288,293	288,293	46,872
Adjusted R-squared	0.433	0.433	0.423

Note: The dependent variable is the net investment probability of establishment i in year t. Estimations are performed by ordinary least squares (OLS). Standard errors (in parentheses) are clustered at the municipality level. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively

Table C.2.2: Event study model: Net investment level

	LBT hike	*Large LBT hike*	*LBT cut*
	(1)	**(2)**	**(3)**
F4	-0.00308	-0.0116	0.0979
	(0.0156)	(0.0288)	(0.0893)
F3	0.00886	-0.00920	0.0557
	(0.0174)	(0.0338)	(0.0648)
F2	-0.00122	-0.0409	0.0744
	(0.0165)	(0.0316)	(0.0871)
L0	0.00514	-0.0103	0.142*
	(0.0171)	(0.0432)	(0.0816)
L1	0.00437	-0.0400	0.124
	(0.0190)	(0.0426)	(0.125)
L2	0.00863	-0.0251	0.211**
	(0.0207)	(0.0453)	(0.107)
L3	0.00415	-0.0153	0.163
	(0.0188)	(0.0449)	(0.123)
L4	0.00207	0.0111	0.0761
	(0.0201)	(0.0427)	(0.107)
L5	0.0162	-0.0226	0.00622
	(0.0177)	(0.0448)	(0.0971)
Constant	12.24***	12.25***	12.36***
	(0.0329)	(0.0166)	(0.0357)
Establishment FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	248,206	248,206	40,869
Adjusted R-squared	0.671	0.671	0.716

Note: The dependent variable is the logarithm of net investment level of establishment i in year t. Estimations are performed by ordinary least squares (OLS). Standard errors (in parentheses) are clustered at the municipality level. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively

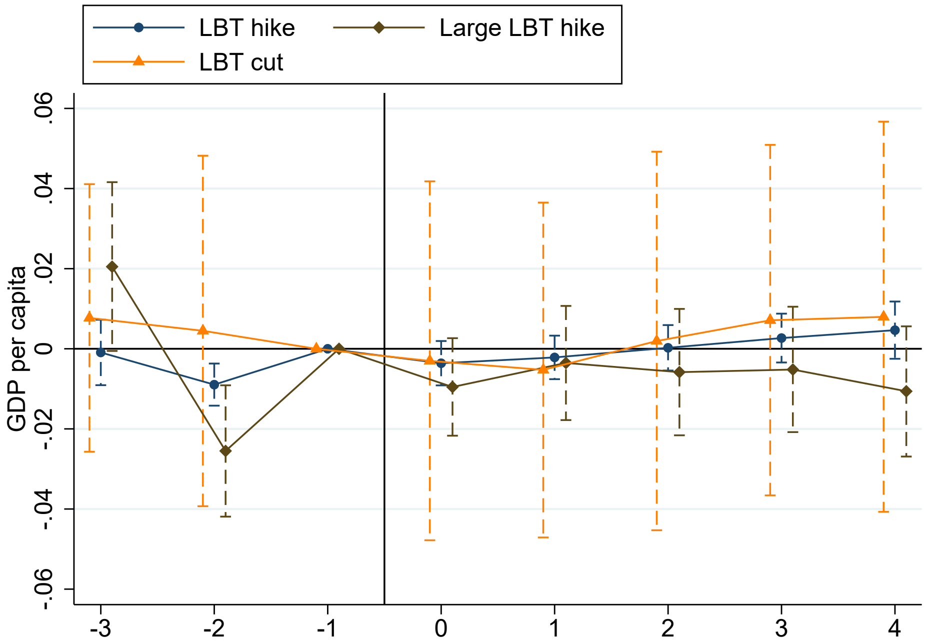
Table C.2.3: Robustness tests: Corporations vs pass-through entities before and after the GBTR 2008

	Prob.	*Prob.*	*Prob.*	*Prob.*	*Level*	*Level*	*Level*	*Level*
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
LBT rate	-0.0751	-0.0266	-0.0130	0.0243	-0.528	-0.526	-0.310	-0.0471
	(0.188)	(0.182)	(0.181)	(0.181)	(1.092)	(1.017)	(1.015)	(1.019)
Corporation	-0.0184	0.00865	0.00802	0.00842	-0.0802	0.0627	0.0571	0.0735
	(0.0258)	(0.0239)	(0.0239)	(0.0240)	(0.169)	(0.156)	(0.156)	(0.156)
Reform08	-0.0448*	-0.0652***	-0.0711***	-0.0782*	-0.205	-0.499***	-0.387**	-0.225
	(0.0238)	(0.0221)	(0.0239)	(0.0419)	(0.149)	(0.141)	(0.154)	(0.369)
LBT rate · Corporation	0.0563	-0.0604	-0.0571	-0.0584	0.363	-0.446	-0.421	-0.483
	(0.162)	(0.152)	(0.152)	(0.152)	(1.075)	(0.987)	(0.987)	(0.987)
LBT rate · Reform08	0.000369	0.0802	0.0547	0.0377	-1.470	-0.345	-0.680	-1.014
	(0.155)	(0.139)	(0.140)	(0.142)	(0.978)	(0.914)	(0.916)	(0.931)
Corporation · Reform08	0.0110	-0.0146	-0.0145	-0.0105	0.109	-0.0127	-0.0135	-0.0255
	(0.0247)	(0.0231)	(0.0231)	(0.0233)	(0.165)	(0.155)	(0.155)	(0.155)
LBT rate · Corporation · Reform08	-0.0201	0.0867	0.0885	0.0540	-0.283	0.250	0.282	0.303
	(0.172)	(0.161)	(0.161)	(0.162)	(1.147)	(1.077)	(1.080)	(1.077)
Establishment controls		✓	✓	✓		✓	✓	✓
Regional controls			✓	✓			✓	✓
Establishment FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Industry-year FE				✓				✓
Observations	362,441	362,441	362,441	362,441	326,687	326,687	326,687	326,687
Adjusted R-squared	0.427	0.463	0.463	0.464	0.702	0.712	0.712	0.713

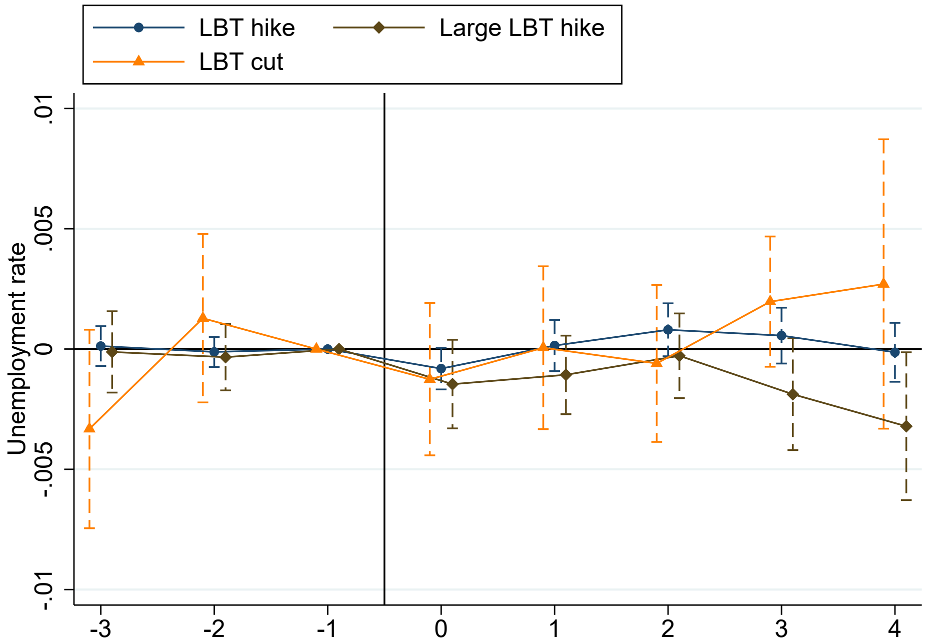
Note: The dependent variables are the logarithm of net investment probability of establishment i in year t in columns (1)–(4) and the net investment level of establishment i in year t in columns (5)–(8). Establishment controls comprise the logarithm of the capital stock and the logarithm of turnover. Regional controls comprise the logarithm of GDP per capita, the unemployment rate, and the logarithm of population. Estimations are performed by ordinary least squares (OLS). We distinguish 15 sectors in the manufacturing industry. Standard errors (in parentheses) are clustered at the municipality level. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

Figure C.2.1: Business-cycle shocks

a GDP per capita



b Unemployment rate



Note: This figure illustrates the estimates and corresponding 95% confidence intervals of the different event study models (i.e., LBT rate hike, large LBT rate hike, and LBT rate cut) for the GDP per capita (Panel A) and the unemployment rate (Panel B). Tables C.2.4 and C.2.5 report the corresponding estimates.

Table C.2.4: Event study model: GDP per capita

	LBT hike	*Large LBT hike*	*LBT cut*
	(1)	**(2)**	**(3)**
F4	-0.00757*	0.0438***	0.0421***
	(0.00395)	(0.0122)	(0.0147)
F3	-0.000913	0.0205*	0.00770
	(0.00416)	(0.0107)	(0.0170)
F2	-0.00895***	-0.0255***	0.00449
	(0.00269)	(0.00837)	(0.0223)
L0	-0.00360	-0.00953	-0.00302
	(0.00283)	(0.00621)	(0.0228)
L1	-0.00215	-0.00353	-0.00526
	(0.00277)	(0.00728)	(0.0213)
L2	0.000247	-0.00583	0.00194
	(0.00289)	(0.00805)	(0.0241)
L3	0.00268	-0.00518	0.00712
	(0.00311)	(0.00797)	(0.0223)
L4	0.00465	-0.0106	0.00797
	(0.00365)	(0.00828)	(0.0248)
L5	0.0134***	-0.00835	-0.0147
	(0.00362)	(0.0105)	(0.0218)
Constant	10.08***	10.06***	10.24***
	(0.00694)	(0.00407)	(0.00610)
Establishment FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	288,293	288,293	46,872
Adjusted R-squared	0.953	0.954	0.975

Note: The dependent variable is the GDP per capita of establishment i in year t. Estimations are performed by ordinary least squares (OLS). Standard errors (in parentheses) are clustered at the municipality level. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively

Table C.2.5: Event study model: Unemployment rate

	LBT hike	*Large LBT hike*	*LBT cut*
	(1)	**(2)**	**(3)**
F4	0.000114	0.00406***	-0.00401*
	(0.000453)	(0.00116)	(0.00210)
F3	0.000123	-0.000121	-0.00332
	(0.000421)	(0.000860)	(0.00210)
F2	-0.000119	-0.000338	0.00128
	(0.000318)	(0.000703)	(0.00178)
L0	-0.000813*	-0.00146	-0.00126
	(0.000441)	(0.000941)	(0.00161)
L1	0.000145	-0.00107	5.35e-05
	(0.000544)	(0.000832)	(0.00172)
L2	0.000800	-0.000279	-0.000598
	(0.000562)	(0.000897)	(0.00166)
L3	0.000560	-0.00188	0.00197
	(0.000592)	(0.00118)	(0.00138)
L4	-0.000134	-0.00321**	0.00270
	(0.000626)	(0.00157)	(0.00307)
L5	0.000550	-0.00411**	0.00781***
	(0.000573)	(0.00165)	(0.00182)
Constant	0.0815***	0.0808***	0.0717***
	(0.000825)	(0.000473)	(0.000847)
Establishment FE	✓	✓	✓
Year FE	✓	✓	✓
Observations	288,293	288,293	46,872
Adjusted R-squared	0.954	0.954	0.962

Note: The dependent variable is the unemployment rate of establishment i in year t. Estimations are performed by ordinary least squares (OLS). Standard errors (in parentheses) are clustered at the municipality level. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively

Table C.2.6: Robustness tests: Alternative dependent variables

	Gross	**Incl. lease**	**Gross**	**Incl. lease**
	Prob.	*Prob.*	*Level*	*Level*
	(1)	**(2)**	**(3)**	**(4)**
LBT rate	-0.0668	0.00891	-0.507	-0.689
	(0.103)	(0.101)	(0.505)	(0.537)
Capital stock	0.0265***	0.0161***	0.0979***	0.0319***
	(0.000561)	(0.000561)	(0.00473)	(0.00300)
Turnover	0.0482***	0.0521***	0.629***	0.663***
	(0.00158)	(0.00158)	(0.0119)	(0.0124)
Constant	-0.0846	0.0412	1.445***	1.553***
	(0.0532)	(0.0553)	(0.291)	(0.334)
Regional controls	✓	✓	✓	✓
Establishment FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Observations	817,372	776,174	710,805	687,487
Adjusted R-squared	0.389	0.359	0.656	0.640

Note: The dependent variables are the gross investment probability of establishment i in year t in column (1), the net investment probability including leased assets of establishment i in year t in column (2), the gross investment level of establishment i in year t in column (3), and the net investment level including leased assets of establishment i in year t in column (4). Regional controls comprise the logarithm of GDP per capita, the unemployment rate, and the logarithm of population. Estimations are performed by ordinary least squares (OLS). Standard errors (in parentheses) are clustered at the municipality level. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

Table C.2.7: Robustness tests: Two-way fixed effects model accounting for continuous treatments

	Prob.	*Prob.*	*Level*	*Level*
	(1)	**(2)**	**(3)**	**(4)**
LBT rate	0.0004	-0.0004	0.0102	0.0064
	(0.003)	(0.012)	(0.024)	(0.855)
Establishment controls		✓		✓
Regional controls		✓		✓
Establishment FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Observations	224,796	224,796	195,292	195,292
Switches	23,957	23,957	20,848	20,848

Note: The dependent variables are the logarithm of net investment probability of establishment i in year t in columns (1)–(2) and the net investment level of establishment i in year t in columns (3)–(4). Establishment controls comprise the logarithm of the capital stock and the logarithm of turnover. Regional controls comprise the logarithm of GDP per capita, the unemployment rate, and the logarithm of population. Estimations are performed by the estimator of De Chaisemartin and d'Haultfoeuille, 2020. Standard errors (in parentheses) are clustered at the municipality level. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

Table C.2.8: Heterogeneity tests: Business characteristics (intensive margin)

	(1)	**(2)**	**(3)**	**(4)**.	**(5)**	**(6)**
LBT rate	-0.463	-0.394	-0.660	-0.653	-0.157	-0.596
	(0.515)	(0.520)	(0.601)	(0.513)	(0.811)	(0.522)
LBT rate · Large estab. (empl.)	0.230***					
	(0.0695)					
LBT rate · Large estab. (turn.)		-0.274				
		(0.440)				
LBT rate · High-prod. estab. (median)			0.377			
			(0.514)			
LBT rate · High-prod. estab. (top 10%)				1.863**		
				(0.753)		
LBT rate · Corporation					-1.002	
					(0.640)	
LBT rate · Estab. of multi-estab. firm						0.519
						(0.422)
Establishment controls	✓	✓	✓	✓	✓	✓
Regional controls	✓	✓	✓	✓	✓	✓
Establishment FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Observations	710,747	710,747	710,747	710,747	326,687	710,747
Adj. R-squared	0.647	0.647	0.647	0.647	0.712	0.647

Note: The dependent variable is the logarithm of net investment of establishment i in year t. Establishment controls comprise the logarithm of the capital stock and the logarithm of turnover. Regional controls comprise the logarithm of GDP per capita, the unemployment rate, and the logarithm of population. Productivity is the ratio of gross profit (turnover less wage expenses) over the total number of employees. Estimations are performed by ordinary least squares (OLS). Standard errors (in parentheses) are clustered at the municipality level. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

Chapter 3

How Does Bonus Depreciation Affect Real Investment? Effect Size, Asset Structure, and Tax Planning

How Does Bonus Depreciation Affect Real Investment? Effect Size, Asset Structure, and Tax Planning

Abstract

We analyze how tax incentives (bonus depreciation) affect real investment choices of firms by exploiting an exogenous variation in regional tax regulation in former East Germany (*Development Area Law*, DAL). Our rich administrative panel data for the universe of German manufacturing firms at the establishment level allow us not only to identify an aggregate effect, but also to identify which types of investment (equipment, buildings, land) are most affected (asset structure). Our baseline results suggest that the DAL increased real gross investment by 16.0% to 19.9%. This aggregate effect is especially driven by additional investments in buildings (76.6% to 92.3%) and land (108.0% to 121.3%), which have the longest regular depreciation periods in absence of bonus depreciation. The impact on equipment investment is significantly smaller (7.3% to 10.5%). Hence, firms did not only increase their real investment, but also adjusted their asset structure in response to the tax incentive. Addressing firm heterogeneity, we observe a stronger response for firms with more than one business establishment and large firms, thereby providing evidence of tax planning opportunities (multi-establishment firms) and relatively low tax planning costs (large firms) enhancing the effect of bonus depreciation on investment. There is only week evidence of financial reporting costs (accounting incentives) moderating the tax induced effect on firms' real investment choices.

JEL classification codes: G11; H25; H32; M41

Keywords: business taxation, bonus depreciation, user cost of capital, tax elasticity, real investment

3.1. Introduction

Policymakers frequently use bonus depreciation to promote investment and foster economic growth. Examples include the 2017 US tax reform, the Dutch bonus depreciation from 2009 to 2011 (Wielhouwer and Wiersma 2017), and the US bonus depreciation from 2008 to 2010. Many OECD countries have frequently used bonus depreciation for counter-cyclical fiscal policy (Maffini, Xing, and Devereux 2019). Our paper addresses the questions (1) whether and to what extent such tax incentive programs affect real investment decisions of firms, (2) how these incentives alter a firm's asset structure, and (3) which firm types react the most. In doing so, we follow several calls for additional research. Hanlon and Heitzman (2010) call for more research on real effects of accounting and tax incentives. Zwick and Mahon (2017) ask for more research regarding the heterogeneity of firm responses. Finally, Jacob (2022) sees a research gap in the relationship of tax effects on investment decisions and tax avoidance opportunities.

While most prior studies find a positive impact on investment from bonus depreciation, the effectiveness of such programs is still under debate (see the discussion in Ohrn 2017; Wielhouwer and Wiersma 2017).[1] Most important, elasticity estimates range widely across studies (4 to 14). In addition, it is unclear whether studies using accounting data are able to fully identify *real* investment responses or could be affected by accounting incentives (e.g., conforming tax avoidance, see Edgerton 2012; Badertscher et al. 2019) and tax avoidance behavior. Furthermore, while Zwick and Mahon (2017) document that average effects can mask large heterogeneity in investment responses to bonus depreciation programs, there is only a small number of papers that investigate this issue (Edgerton 2010; Wielhouwer and Wiersma 2017; Zwick and Mahon 2017). Edgerton (2012) argues that the effectiveness of investment tax incentives depends on accounting incentives. He hypothesizes that programs that provide tax benefits and also increase book income (e.g., investment tax credits) might have a stronger effect on investment activity than programs that do not have a positive effect on book income (e.g., bonus depreciation). We are not aware on any current research that discusses the link between accounting incentives, tax avoidance opportunities and the effectiveness of investment tax incentives.

We use an exogenous variation in a German bonus depreciation program (Development Area Law, DAL) to address these important questions. The first target is to identify and quantify the average effect on real investment activity of treated German establishments by a difference-in-differences (DiD) estimation strategy. A main benefit of our data is that we can observe real investment data in a mandatory survey of the German

[1] On the one hand, there is evidence of a positive effect of bonus depreciation on investment (House and Shapiro 2008; Edgerton 2012; Maffini, Xing, and Devereux 2019; Zwick and Mahon 2017; Wielhouwer and Wiersma 2017; Ohrn 2019). On the other hand, Cohen and Cummins (2006), Dauchy and Martínez (2008), Hulse and Livingstone (2010), and Edgerton (2011) do not find corresponding evidence.

Federal Statistical Office at the establishment level that will not be affected by accounting choices like earnings management or conforming tax avoidance. We consider subsidized establishments in East German states as the treatment group and non-subsidized establishments in the West states as the control group and compare investments in the DAL treatment period (1995–1998) with investments in the period after expiration of the DAL (1999–2008). In 1995, the first year in our sample, the vast majority of establishments in the East states were owned by West German firms and had access to the same technologies.[2] Thus, we expect West German establishments to be an appropriate control group.[3] Our baseline estimates suggest a treatment effect on investment at the extensive margin (investment probability) of 3.8 percent to 4.4 percent and at the intensive margin (conditional investment volume) of 11.7 percent to 14.8 percent, suggesting an aggregate effect of 16.0 percent to 19.9 percent. If we also consider a wide range of additional robustness checks, the effect size ranges from 10.5 percent (for a balanced panel of establishments over the whole observation period) to 34.8 percent (for a mixed panel of firms with establishments in both parts of Germany). Hence, our results point to bonus depreciation programs having substantial effects on real investment activity.

Going beyond identifying an aggregate effect, we investigate two dimensions of heterogeneity: capital types and firm characteristics. From a theoretical perspective, bonus depreciation becomes more valuable for investment goods with long regular depreciation benefits (e.g., buildings). While existing empirical research often relies on that assumption (e.g., Desai and Goolsbee 2004; Edgerton 2010; Zwick and Mahon 2017), corresponding evidence is still missing. We find that the investment response for assets with long regular depreciation periods like building investments (76.6 percent 92.3 percent) and land investments (108.0 percent to 121.3 percent) by far exceeds the investment response of equipment investments (7.3 percent to 10.5 percent). This finding is robust for a wide range of tests and underlines that investment tax incentives do not only affect investment volumes but also the firms' asset structure.

Regarding firm heterogeneity, we expect that the effectiveness of bonus depreciation increases in opportunities for subsidy shopping, i.e. tax planning, and decreases in tax planning costs (e.g., Jacob 2022). Due to economics of scale in tax avoidance (Hundsdoer-

[2] The main privatization strategy of the *Treuhand* Agency (THA) at the end of the German Democratic Republic was to find an experienced and established West German or international firm, while management-buy-outs were a secondary alternative (Paqué 2009, p. 47f.). As the fraction of international investors was only about 6 percent, experienced firms in the former West were by far the most relevant investor group (BvS, 2003, p. 58).

[3] Note that our approach does not require identical economic conditions in both parts of Germany, since we control for the district-level economic situation. We also account for differences in cross-sectional characteristics by establishment fixed effects and other control variables. In robustness checks, we rely on firm panels with establishments in both parts of Germany and propensity score matching to increase the similarity of control and treatment groups. Our analyses provide compelling evidence for common trends in the investment activity of both groups in the post-DAL period – our reference point.

fer and Jacob 2019), tax planning costs should decrease in firm size, thereby increasing the effectiveness of bonus depreciation for large firms. Our analysis provides robust evidence that multi-establishment firms and large firms react more strongly to the bonus depreciation. We also address the relevance of accounting incentives. Due to special accounting regulations, the use of the German bonus depreciation in the tax return required an equal depreciation deduction in the financial accounts ("reversed" book-tax conformity). As a consequence, the reform produced financial reporting costs by impairing the information content of financial accounts and by forcing firms to under-report their earnings. While we hypothesize that firms with higher financial reporting costs should show a weaker investment response in line with Edgerton (2012), we do not find such evidence. Thus, accounting incentives to not seem to play an important role for the effectiveness of bonus depreciation programs.

Our paper contributes to the literature in a number of ways. First, we contribute to the literature on the effectiveness of bonus depreciation programs (e.g., Desai and Goolsbee 2004; Edgerton 2010; Zwick and Mahon 2017). While Eichfelder, Jacob, and Schneider (2023) use the same policy variation to analyze the effect of the German bonus deprecation on the quality of investments, our focus is on identifying the impact of the program on the quantity of real investments. A benefit for our study is that we rely on an exogenous policy variation identified at the establishment level (different from most studies which identify policy variation at the industry level) and have access on high quality data from the German Federal Statistical Office on real investments. Thus, our analysis will remain unaffected by tax avoidance and earnings management activities that could bias accounting data (e.g., Badertscher et al. 2019; Eichfelder et al. 2023. Our estimates suggest an elasticity of the user-cost of capital to the German bonus depreciation program ranging from 4 to 5 for our baseline estimates and from 2.7 to 8.1 if we consider additional robustness checks. This elasticity is moderate if we compare it to existing evidence on bonus depreciation programs (elasticity range of 6 to 14, see House and Shapiro 2008; Maffini, Xing, and Devereux 2019) but high if we compare it to investment elasticities with regard to tax *rates* (elasticity range of 0.2 to 1, see Auerbach and Hassett 1992;Chirinko, Fazzari, and Meyer 1999; Bond and Xing 2015; Melo-Becarra, Mahecha, and Ramos-Ferrero 2021 with further references).

Second, as we have access to detailed administrative data regarding investment types at the establishment level (e.g., Desai and Goolsbee 2004; Edgerton 2010; Zwick and Mahon 2017), we are the first to estimate the impact of bonus depreciation on asset structures. While most studies use the industry level variation in tax benefits for different assets to identify the investment reactions of firms, empirical evidence of a stronger investment response for assets with long standard depreciation periods is still missing.

Third, we contribute to the scarce literature on how different firm types react to bonus depreciation incentives (Edgerton 2010; Zwick and Mahon 2017; Wielhouwer and Wiersma 2017). We follow the advice of Jacob (2022) and are the first to analyze the relationship of investment tax incentives, planning costs and tax avoidance opportunities. We find that large firms with lower tax planning costs reacted considerably stronger to the tax policy. This is consistent with evidence from Knittel (2007) and Kitchen and Knittel (2011) on lower take-up rates of bonus depreciation by small firms in the United States.[4] We also find a stronger investment reaction of multi-establishment firms with higher opportunities for subsidy shopping. By contrast, we do not find evidence that accounting incentives and conforming tax avoidance (e.g., Edgerton 2012; Badertscher et al. 2019; Eichfelder et al. 2023) play a relevant role for the effectiveness of bonus depreciation programs.

The remainder of the paper is structured as follows. Section 3.2 describes the German investment tax incentives and corresponding accounting regulations. Section 3.3 introduces the theoretical framework and derives the hypotheses. We describe the identification strategy and data in Section 3.4. Section 3.5 presents the results, while Section 3.6 concludes.

3.2. Institutional Background and Development Area Law (DAL)

Corporate profits in Germany are subject to corporate income tax, local business tax, and to dividend taxes upon distribution.[5] The German tax code defines the tax base, including depreciation schemes applicable to all firms in Germany. In 1991, the German federal government enacted the Development Area Law (DAL) bonus depreciation program to foster business investment in the five East states (Brandenburg, Mecklenburg-West Pomerania, Saxony, Saxony-Anhalt, and Thuringia) and Berlin. In addition to DAL, firms could also apply for tax-exempt grants from the Investment Subsidy Law (German: *Investitionszulagengesetz*, expired in 2013, hereafter ISL) and taxable grants of the Joint Task Program "Enhancement of Regional Economic Structure", which supports investments in underdeveloped German areas (German: *Gemeinschaftsaufgabe "Verbesserung der regionalen Wirtschaftsstruktur"*, still ongoing, hereafter JTP). The DAL was among the most costly subsidies of the 1990s and the only program that included bonus depreciations. In 1996,

[4]Our findings are not necessarily a contradiction to Zwick and Mahon (2017), since the definition of firm size in our analysis significantly differs from their paper. Zwick and Mahon (2017) interpret their finding of a larger investment response of smaller firms as evidence that liquidity constraints increase the effectiveness of investment tax incentive programs.

[5]From 1999 to 2001, there were major reforms of the German tax system affecting corporate taxation (with a large reduction of the German corporate income tax burden) and dividend taxation (with a large reduction of the dividend tax). As the reform affected corporations in both parts of Germany in a similar way, our DiD design effectively accounts for such endogeneity concerns.

the DAL ranked first among all German tax incentive programs.

The DAL allowed firms to depreciate 50% of eligible investments immediately, while the remaining 50% of book value were depreciated over the useful asset life.[6] The bonus depreciation could be easily claimed in the filing of the regular tax return and was not restricted to specific branches or business types.[7] It was available for all movable assets (except for aircrafts) and for structures (including the modernization of buildings). We exploit the expiration of the DAL in December 1998, which we interpret as an increase in the user costs of capital.

An interesting aspect for our analysis is the accounting treatment of the DAL bonus depreciation. Before the German Accounting Law Modernisation Act from 2008 (German: *Bilanzrechtsmodernisierungsgesetz*), the German GAAP had a special form of book-tax conformity that required a consideration of special tax treatments in financial accounts (so-called "reversed" book-tax conformity). Therefore, if a firm wanted to save taxes by using bonus depreciation in the tax accounts, it also had to report lower book income. For example, if a firm wanted to deduct a bonus depreciation of € 1 million from taxable profit, the earnings in the financial reports of the same year were also reduced by a depreciation of € 1 million. Hence, the use of bonus depreciation resulted in potential misinformation in the financial reports and corresponding financial reporting costs.[8]

Table 3.1 summarizes the most relevant features of the programs in place. Note that the key changes occurred for bonus depreciation (DAL) and the DAL was the only program comprising such tax incentives.

Figure 3.1 shows the aggregate value of DAL ISL, and JTP subsidies by their present value (for computational details see Online Appendix A) from 1995 to 2008. Aggregate subsidy volumes (DAL, ISL, and JTP) as well as DAL subsidies dropped significantly around the DAL expiration in 1998/1999, while the sum of ISL and JTP subsidies remained stable over time. The small DAL subsidies after 1998 resulted from delayed bonus

[6]As an alternative, bonus depreciation could have been freely allocated over the first five years following the investment if no other special depreciation schemes had been used.

[7]In contrast to the DAL, JTP and ISL required a formal application, resulting in higher compliance costs. The assessment base of both programs was smaller and funding criteria were more rigid. Before 1999, ISL grants were restricted to new movable assets, with some exceptions (no low-value assets, cars, or aircraft). After 1999, ISL grants were expanded to new structures, but only in the case of so-called "initial" investments, including the foundation or extension of an establishment, major modifications of products and production methods, and the acquisition of a business that would otherwise have been liquidated. In case of the JTP, fundable investments included movable and intangible assets. Different from DAL and ISL, there was no legal entitlement for JTP grants. Thus, the success of applications and funding rates depended on the individual decisions of administrative authorities.

[8]In contrast to that, direct and tax-exempt ISL subsidies were regarded as tax-exempt income in the financial accounts that increased earnings. JTP grants could be either reported as taxable earnings or as a reduction of acquisition costs in their financial reports. This resulted either in higher income in the current year (by higher earnings) and/or in future periods (by lower depreciations). Therefore, while DAL bonus depreciation had a negative effect on current earnings, ISL and JTP subsidies had either a neutral or a positive effect on the income of the current year.

Table 3.1: Regional investment subsidies for establishments in Eastern Germany, 1995–2008

	DAL	ISL	JTP
Validity period	Until December 31, 1998	Whole observation period	Whole observation period
Subsidy form	Bonus depreciation	Direct and tax-exempt subsidy	Direct and taxable grant
General rates	50% (1995–1996), 40% (1997–1998)	5% (1995–1998),c 10% (1999), 12.5% (since 2000)	Maximum rates (actual grants depend on authority decision and overall funding level): 35% (1995–1996); 28–35% (1997–2006); 30% (since 2007)
Increased rates	NA	+ 5% (Small and medium-sized enterprises, SME, 1995–1998), twice the general rate for initial equipment investment (SME, since 1999); + 2.5% (border areas, since 2001)	Additional maximum rates for small and medium-sized enterprises: +15% (1995–2006); +10–20% (since 2007)
Special regional regulations	NA	Berlin: reduced validity periods (West Berlin) and reduced rates under certain conditions	Maximum rates and detailed regulations depend on the regional area; reduced rates for Berlin area (since 2000)
Assessment base	Movable assets (excluding aircraft), immovable assets, modernization of buildings	New and movable assets (excluding low-grade assets, aircraft, cars), new and immovable assets (since 1999), restriction to initial investments (since 1999)	Movable assets and intangible assets; fundable investments depend on minimum investment volumes, employment effects, and authority decisions
Formal requirements	Tax return with legal entitlement	Formal application with legal entitlement	Formal application without legal entitlement

Note: This table summarises the 3 major subsidies that were in place during the sample period 1995-2008. The last amendment of the law (ISL 2010) had run out by the end of 2013. The investment subsidy rate is up to 8% until the end of 1996 for investments that started before July 1994.

depreciations. Taken together, Figure 3.1 documents a strong and permanent decline in aggregate tax incentives for investments in East establishments due to the expiration of the DAL program.

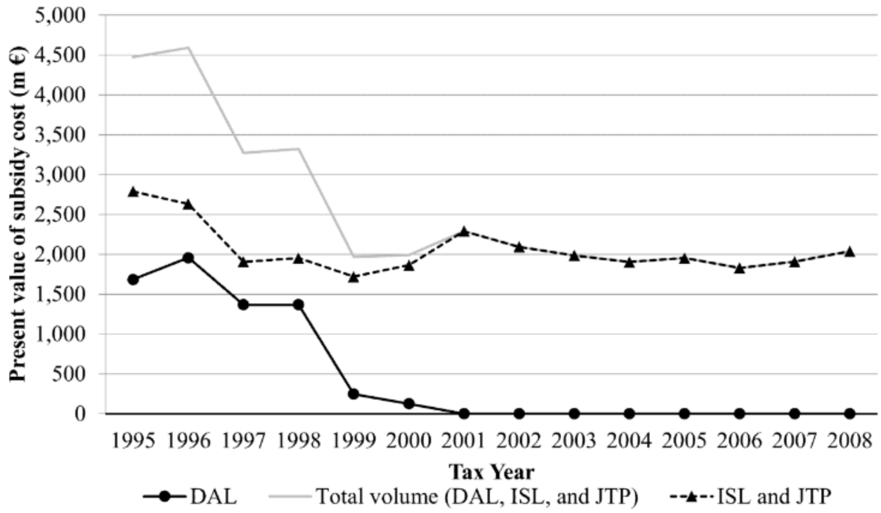


Figure 3.1: Subsidy volume of DAL, ISL, and JTP

Note: This figures plots the present value volumes of subsidy costs for the DAL program and the sum of the DAL and ISL programs, based on German government reports on subsidies (*Deutscher Bundestag, Drucksache 12/1525, Drucksache 13/2230, Drucksache 14/1500, Drucksache 15/1635, Drucksache 16/6275*). For the calculations, see Appendix A.

3.3. Theoretical Framework and Hypotheses

The seminal work of Hall and Jorgenson (1967) presents the general framework for the impact of tax policy on business investments. According to their model, taxes and tax incentives have an impact on the user cost of capital. Abstracting from adjustment costs, the user cost of capital is (e.g. Cohen, Hansen, and Hassett 2002; Devereux and Griffith 2003)

$$C_t = \varphi_t \cdot T_t \cdot [\rho_t + \delta_t - E(\Delta\varphi_t/\varphi_t)], \tag{3.1}$$

with φ_t representing the price level, ρ_t the after-tax cost of funds, and δ_t the physical rate of depreciation at time t. $E(\Delta\varphi_t/\varphi_t)$ describes expected changes in the price of capital goods. Therefore, $\delta_t - E(\Delta\varphi_t/\varphi_t)$ is the expected net rate of depreciation (Auerbach 1983). The tax term is defined as

$$T_t = \frac{(1 - \tau_t \cdot Z_t - s_t)}{(1 - \tau_t)}, \tag{3.2}$$

where τ_t is the tax rate on profits, s_t is the rate of direct subsidies (in our case ISL and JTP benefits), and Z_t is the present value of depreciation allowances per € invested (in our case the sum of regular depreciation benefits plus bonus depreciation benefits).

Considering the strong decline in aggregate subsidy volumes in 1998 as documented by Figure 3.1, we conclude that the expiration of the DAL increased the average user costs of capital of treated establishments and hypothesize abnormally high investments in these establishments during the DAL treatment period.

H1: *DAL increased real investment in treated establishments.*

Further, we address the heterogeneity of the investment response. As documented by Eq. 3.1, the user costs of capital decrease in depreciation benefits $\tau_t \cdot Z_t$. In case of an immediate write-off (bonus depreciation), the value of the depreciation benefits is equal to τ_t as Z_t is one for $t = 0$ (see also Cohen, Hansen, and Hassett 2002). Therefore, bonus depreciation becomes more valuable if regular depreciation benefits are small. For $t \to \infty$ the present value of depreciations Z_t converges to zero, which is the case for assets with very long depreciation periods like unbuilt ground or structures. Therefore, we expect a stronger impact of the bonus depreciation program on assets with long depreciation periods like building and land investments.

H2: *DAL had a stronger effect on investment goods with long standard depreciation periods (buildings and land).*

We further analyze how firm characteristics are related to the effectiveness of the DAL bonus depreciation program. First, we account for the fact that tax avoidance and tax planning is a costly activity that also depends on opportunity. The literature on tax complexity and compliance costs clearly documents that the marginal costs of tax planning and compliance decrease with firm size (Gunz, Macnaughton, and Wensley 1995; Richardson and Lanis 2007). The combined planning and compliance costs as a fraction of sales revenue can be 10 times or even 100 times larger for small firms than for large firms (Eichfelder and Vaillancourt 2014). Therefore, larger firms tend to spend more resources on tax planning and the optimization of tax benefits. In line with that argument, Knittel (2007) and Kitchen and Knittel (2011) observe low take-up rates of bonus depreciations and accelerated depreciations by small US businesses.

The literature on profit shifting and tax avoidance further documents that business groups with international subunits relocate patents, earnings or costs with the target to generate tax benefits (e.g., Dharmapala 2014; De Simone, Klassen, and Seidman 2017; Hundsdoerfer and Jacob 2019). In similar terms, we might expect that firms with more than one establishment have more opportunities to adjust their investment strategy in order to benefit from bonus depreciation ("subsidy shopping"). Both arguments suggest:

H3a: *The DAL had a stronger effect on investments of firms with low planning costs (large firms) and on investments of firms with more opportunities for subsidy shopping (multi-establishment firms).*

An interesting institutional aspect of the German DAL regime is its accounting treatment. As documented in Section 3.2, the use of the bonus depreciation in the tax return required an equal depreciation deduction in the financial accounts ("reversed" book-tax conformity). As a consequence, while bonus depreciation provided cash tax benefits, it also produced financial reporting costs by impairing the information content of financial accounts and by forcing firms to underreport their earnings. Edgerton (2012) hypothesizes that firms do not only consider tax benefits but also financial reporting incentives of investment tax benefits in their investment decisions (see also Aarbu and MacKie-Mason 2003; Klemm 2010). Therefore, we expect that the investment response to the DAL is partially offset by the financial reporting costs induced by "reversed" book-tax conformity.

We use two proxies for these costs. First, due to accountability and the capital maintenance principle,[9] corporations likely face higher financial reporting costs. Second, we expect that owner-managed firms have smaller financial reporting costs, since they are less affected by agency problems.

H3b: *The DAL had a stronger effect on firms with low financial reporting costs (non-corporate firms and owner-managed firms).*

3.4. Identification Strategy and Data

3.4.1. Identification Strategy

We interpret the expiration of the DAL as a natural experiment. Since establishments in the West German states were not subsidized, we use them as a control group and identify the DAL effect by difference-in-differences (DiD) estimation. An important advantage of our identification strategy is that the DAL bonus depreciation applied to almost all types of investments in the East states. It was not necessary to apply for DAL or to actively "self-select" into the DAL program. Hence, self-selection should be of minor concern (see e.g., Wielhouwer and Wiersma 2017 for related problems). To control for potential self-selection by founding an establishment in a treatment area, we also perform a robustness check in Section 3.5.3 for a balanced panel of establishments that existed at the beginning of our observation period (1995).

A main prerequisite for DiD estimation is the common trends assumption.[10] We

[9]In Germany, limited liability companies and other corporate firms are only allowed to distribute their (retained) book income after taxes as a dividend. In addition, such corporate firms are typically less closely held than partnerships and sole proprietorships, thereby increasing agency problems and related financial reporting costs. We therefore expect a weaker investment response of corporations.

[10]Another relevant but often neglected prerequisite is the stable unit of treatment assumption (SUTVA). SUTVA implies that treatment affects the treatment group but not the control group. A potential concern in our case might be that investments could have been redirected from the West to the East in order to obtain higher bonus depreciation. However, considering the dominance of the West German economy and the limited size of the economy in the East part of Germany, this should be a minor problem. In spite of the bonus depreciation from 1995 to 1998, only about 14.5% of the investments in our data were conducted in East Germany. Thus, even if some of these investments were shifted from the West, this could have

discuss potential concerns regarding this assumption in Subsection 3.4.4. Most importantly, graphical analysis in this subsection provides compelling evidence of a common trend after the expiration of the DAL bonus depreciation as well as a structural break after the expiration of the program.

We account for potential differences between establishments in both parts of Germany in several ways. First, we control for time-invariant differences by establishment fixed effects, α_i. Second, we capture differences in capital stock, productivity, and general economic conditions in the region (e.g. unemployment rates, GDP per capita) by a set of control variables, X_{it}. Third, to account for economic shocks, we also include year fixed effects, γ_t, and industry-year fixed effects, θ_{it}. Fourth, in robustness checks in Section 3.5.3 and additional analyses in Section 3.5.5 we perform tests for a sample of firms with establishments in both parts of Germany and also apply propensity score matching to make our control group more similar to our treatment group. Fifth, we provide estimates for a wide array of additional specifications with robust results. Our baseline model can be written as

$$I_{it} = \beta_0 + \beta_1 \cdot DiD_{it} + \phi \cdot X_{it} + \alpha_i + \gamma_t + \theta_{it} + \epsilon_{it}. \tag{3.3}$$

We use two alternative dependent variables, I_{it} , for aggregate gross investments at the extensive and intensive margins of establishment i at time t (H1). We measure I_{it} using either a dummy variable indicating whether a firm has invested or not (extensive margin) or the logarithm of (positive) investment volume (intensive margin). In additional analyses (Subsection 3.5.2), we also consider investment measures for different asset types (H2). Similar to Zwick and Mahon (2017), we rely on the logarithm of investment to measure investment at the intensive margin, which allows us to interpret coefficients as elasticity estimates.[11]

The variable of interest in Eq. (3.3) is DiD_{it}, which is the interaction term of a dummy variable for establishments in East Germany and a dummy for the DAL treatment period. Therefore, DiD_{it} has a value of one if establishment i is located in an East state and the observation is before 1999. We identify the average treatment effect by β_1. Since year and establishment fixed effects are included, the dummy variables $East_i$ and $Before99_t$ are redundant. At the establishment level, our vector of controls, X_{it}, includes the logarithm of the capital stock from the preceding period, K_{it-1}, as a proxy for capital endowment. We proxy the investment potential using the ratio of revenue to K_{it-1}. This ratio also serves as a measure for capital constraints, since revenue is positively correlated with cash flows as a common proxy for capital constraints (Hadlock

had only a minor impact on the investment activity of our control group (i.e., the establishments in the West states).

[11]In an unreported robustness check, we also use investments scaled by capital stock as an alternative dependent variable with consistent results.

and Pierce 2010). Controlling for economic conditions at the district level, we consider the unemployment rate, the logarithm of the price-adjusted GDP per capita, and the logarithm of the population in a district.

To test hypotheses H3a and H3b, we add triple difference interaction terms to Eq. (3.3). These terms interact the indicator DiD_{it} with the following dummy variables for firm characteristics: $Large_{it}$ is a dummy with a value of one for large firms with at least 250 employees; $Group_{it}$ is a dummy with a value of one for multi-establishment firms with more than one establishment; $Owner_{it}$ is a dummy with a value of one for firms with an active business owner in the management of the firm; $Corp_{it}$ is a dummy with a value of one for corporate firms with limited liability. We obtain:

$$\begin{aligned} I_{it} = \beta_0 &+ \beta_1 \cdot DiD_{it} \\ &+ \beta_2 \cdot DiDLarge_{it} + \beta_3 \cdot DiDGroup_{it} + \beta_4 \cdot DiDOwner_{it} + \beta_5 \cdot DiDCorp_{it} \\ &+ \beta_6 \cdot Large_{it} + \beta_7 \cdot Group_{it} + \beta_8 \cdot Owner_{it} + \beta_9 \cdot Corp_{it} \\ &+ \phi \cdot X_{it} + \alpha_i + \gamma_t + \theta_{it} + \epsilon_{it}. \end{aligned} \tag{3.4}$$

In Eq. (3.4), β_1 captures the average DAL effect, while β_2, β_3, β_4, and β_5 capture the additional effects from large, multi-establishment, owner-managed and corporate firms. Thus, the overall effect for firms with all characteristics is the aggregate effect of all coefficients β_1 to β_5.

3.4.2. Data

Our analysis uses the German AFiD panel (German: *Amtliche Firmendaten in Deutschland*) for the manufacturing and mining industries from 1995–2008, which includes a number of mandatory business surveys conducted by the German Federal Statistical Office.[12] The main surveys used in this analysis are the Investment Survey and the Monthly Report for the manufacturing and mining industries.[13] Both surveys are a census of the universe of business establishments in these sectors with at least 20 staff members, including managers and working business owners, and provide information at the establishment level. We also collect data at the district level (GDP per capita, population, unemployment rate) from RegioStat[14] to control for regional economic conditions. Hence, we have a comprehensive panel of establishments covering the period between 1995 and 2008.

Compared to firm panels from Compustat or AMADEUS, AFiD has clear advantages for our analysis. First, unlike accounting data, the Investment Survey provides

[12] The data can only be accessed by remote data processing (Malchin and Voshage 2009).

[13] In German: *Investitionserhebung bei Betrieben des Verarbeitenden Gewerbes sowie der Gewinnung von Steinen und Erden; Monatsbericht bei Betrieben des Verarbeitenden Gewerbes sowie der Gewinnung von Steinen und Erden.*

[14] See also `https://www.regionalstatistik.de/genesis/online/logon` (20.07.2023).

information at the most granular level of identification, the business establishment. This is crucial for our analysis, since we need corresponding data for a clear identification of DAL-treated investments. Note that establishments in the East typically belong to West firms.

Second, as AFiD is a mandatory business survey for the universe of establishments in the German manufacturing sector, non-response, self-selection or a potential lack of representativeness, which are common problems in accounting research, are not challenges for our analysis. In addition, the very detailed information in our data allow us to disentangle investment responses for different types of assets, namely equipment, buildings and land, which is typically not possible by the use of accounting and aggregate data.

Lastly, our measures for business investment will not be affected by depreciation policies, earnings management or conforming tax avoidance (see for example Dobbins et al. 2018; Badertscher et al. 2019; Eichfelder et al. 2023), while we have data on investment flows instead of stocks. These conditions make it feasible to investigate the effect of bonus depreciation on *real* investment activity.

A potential disadvantage is that our data is restricted to the manufacturing sector, which, however, is a very relevant part of the German economy. In addition, since there is no financial reporting at the establishment level, the data does not provide explicit information on capital stocks. Therefore, extending the approach of Wagner (2010), we estimate the capital stock at the establishment level using information from the Cost Structure Survey[15] (for computational details see Appendix B).

The raw data comprises 691,822 establishment-year observations. Due to the special status of the Berlin area, we omit the 13,394 observations located in Berlin. We also drop 21,019 observations of mining companies. Finally, we drop 113,324 observations with incomplete information on our primary variables of interest (e.g. resulting from business restructurings or restructurings at the county level). After these adjustments, our sample comprises 544,085 observations over 14 years.[16]

We price-adjust the data on investments, sales, and capital stocks. Since the German Federal Statistical Office does not report regional producer price indices, we use the German Producer Price Index for the manufacturing industry (Bofinger et al. 2011). Building prices, however, depend on local economic conditions. Since a regional subsidy like bonus depreciation can affect regional prices (Goolsbee 1998; House and Shapiro 2008), we use state-level building price indices for the manufacturing industry to control for price differences of building investments (see also Appendix C).

[15] German: *Kostenstrukturerhebung*

[16] Note that due to M&A and other forms of restructuring, a single establishment may be owned by more than one firm over the time period studied.

3.4.3. Descriptive Statistics

We report descriptive statistics of our sample in Table 3.2. On average, price-adjusted gross investments in the control (West) group (€ 1,140.45 thousand) slightly exceed the values in the treatment (East) group (€ 1,021.27 thousand). While average equipment investment and land investment per establishment are slightly larger in the control group, average building investment is somewhat larger in the treatment group. The percentage of establishments with positive gross investments is quite high and almost identical in the treatment group (85.68 percent) and the control group (86.64 percent). Establishments in the East have a higher probability of investing in buildings and land (28.40 percent and 5.11 percent, respectively) compared to West establishments (19.59 percent and 3.00, respectively). This is in line with our expectation that the DAL especially promoted investments in assets with long depreciation periods. West establishments have larger revenues and larger capital stocks than East establishments. This is in line with representative balance sheet data provided by the German Central Bank (Bundesbank 2012), according to which the ratio of revenue to capital stock of East German firms is smaller than in the West. Unemployment rates in East German districts are higher and GDP-per-capita ratios are smaller than in West districts.[17]

3.4.4. Common Trends Assumption

The key assumption critical to our identification strategy is the common trends assumption. Apart from the treatment effect, the trends of the two samples (the treatment group and the control group) should not differ from each other. In our study, we examine the expiration of a bonus depreciation regime. We interpret the DAL's expiration as a policy change that *increases* the user cost of capital of investments in East establishments, relative to West establishments. Consistent with standard DiD estimation, we expect a common trend of investment activity in both parts of Germany after the change in the user costs. Before the DAL's expiration, H1 suggests a positive treatment effect on the volume of investments in East establishments.

[17]We also use propensity score matching as a robustness check to make the control and treatment groups more comparable. Appendix D provides details on the matching process, while Table D.3.2 contains results of estimating the baseline Eq. (3.3) with the matched sample. The results remain qualitatively and quantitatively unchanged.

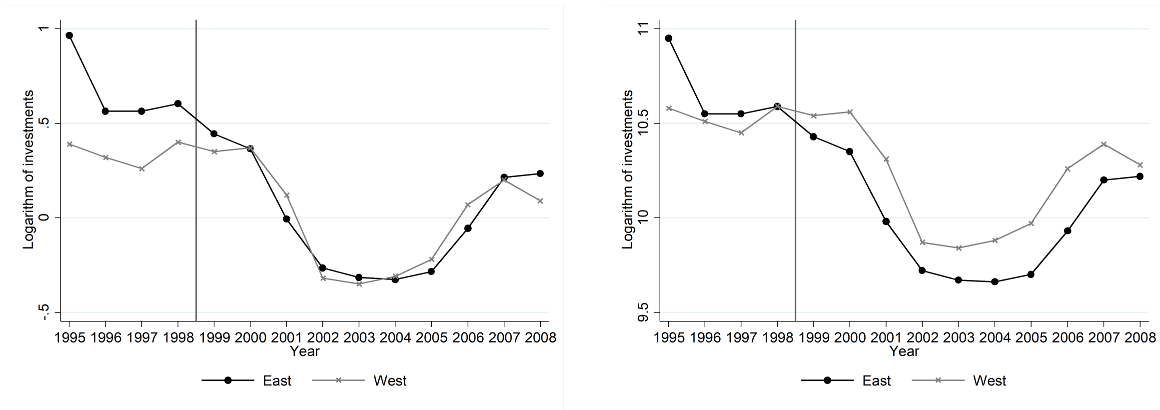
Table 3.2: Descriptive Statistics by Region

	Full Sample (N=544,085)			**West Germany (N=456,913)**			**East Germany (N=87,172)**		
Variable	Mean	SD	P50	Mean	SD	P50	Mean	SD	P50
Real investments (thousands €)									
Gross investment	1,121.36	10,947.59	111.28	1,140.45	11,243.91	113.46	1,021.27	9,239.69	99.70
Equipment investment	983.56	10,041.45	100.00	1,007.27	10,338.92	103.54	859.32	8,308.80	82.51
Building investment	129.97	1,498.17	0.00	125.07	1,384.05	0.00	155.65	1,949.36	0.00
Land investment	7.83	271.95	0.00	8.12	293.23	0.00	6.30	104.41	0.00
Fraction of establishments with positive investments (%)									
Gross investment	86.48	34.19	100.00	86.64	34.02	100.00	85.68	35.03	100.00
Equipment investment	86.10	34.59	100.00	86.28	34.40	100.00	85.15	35.56	100.00
Building investment	21.00	40.73	0.00	19.59	39.69	0.00	28.40	45.09	0.00
Land investment	3.34	17.97	0.00	3.00	17.07	0.00	5.11	22.02	0.00
Control variables									
Capital stock (thousands €)	5,104.99	39,896.66	1,375.64	5,200.81	41,021.10	1,354.22	4,602.75	33,384.80	1,488.71
Revenue (millions €)	29.90	296.16	5.77	32.84	321.33	6.25	14.48	77.32	3.87
Revenue per capital (%)	174.60	45,913.49	4.24	205.06	50,099.93	4.56	14.97	1,084.40	2.60
GDP per capita (thousands €)	25.01	9.72	23.07	26.49	9.76	24.03	17.27	4.33	16.21
Population (thousands)	273.86	239.69	202.20	297.87	251.63	241.77	148.02	88.55	130.30
Unemployment rate (%)	10.52	4.57	9.20	9.01	2.92	8.50	18.43	3.32	18.40

Note: Descriptive statistics of the main variables (see Section 4) for the AFiD panel of establishments in the manufacturing sector in Germany 1995-2008; Panel A contains the full sample, while Panel B shows Western establishments (control group), and Panel C depicts Eastern establishments (treatment group). All investment and regional variables are price-adjusted using the German Producer Price Index. Building investments are price-adjusted using building price indices from the German states (see Appendix C).

A potential concern regarding the common trends assumption might be general differences in development or business cycles of the establishments in both parts of Germany. Note that our analysis does not require a common trend of both parts of Germany but rather a common trend of the investment activity of establishments in the manufacturing sector in both parts of Germany. As mentioned before, establishments in the former East in the middle of the 1990s were typically owned by West German firms. Thus, they competed in the same market, had typically the same owner, and access to the same technologies as their West counterparts.

In order to provide evidence on common trends, we compare the investments for the treatment and control groups for the years before and after the annulation of the DAL program (period from 1995 to 2008) graphically. Since we are only interested in differences in trends for both groups and not in differences in means, we de-mean all the variables with their average value in the period after 1999 and subtract the mean of the logarithm of investments from the post-DAL period. Hence, we calculate yearly deviations from the "normal" average investment activity from 1999 to 2004. Figure 3.2 shows the average price-adjusted and de-meaned gross investments for the treatment and control groups. Prior to 1999, the treated establishments have an abnormally high level of investment, as one would expect due to the bonus depreciation regime. In addition, we find some graphical evidence that the investment reaction to the DAL expiration took about one year to converge to the normal level in 2000. This is not unexpected, since delays in building projects and construction works are a common problem. Thus, even if firms intended to reduce their investment activity after the DAL expiration, this reaction to the tax-driven increase in the user costs of capital was likely to take some time.



a De-Meaned investments East and West

b Investment levels East and West

Figure 3.2: De-meaned investment and investment levels before and after treatment

Notes: Panel (a) plots the de-meaned value of the logarithm of gross investments in the manufacturing sector for the treatment group (East German establishments) and the control group (West German establishments). The figure highlights the trend in the investment activity of both groups in the DAL period (1995–1998) and following years. Panel (b) does the same for investment levels.

3.5. Results

3.5.1. Average Treatment Effects

Our analysis starts with estimating the baseline model in Eq. (3.3) for gross investments at the extensive and intensive margins. For investments at the extensive (intensive) margin, the dependent variable is a dummy variable with a value of one for establishments having non-zero gross investment (the natural logarithm of price-adjusted gross investment). When modelling investments at the intensive margin, we only consider observations with positive investments, thus reducing the number of observations. Our final sample includes 544,085 observations of 68,289 establishments for investments at the extensive margin and 470,548 observations of 63,733 establishments for investments at the intensive margin.

Since we are not interested in predicting investments, but rather explaining them, we rely on a linear probability model to investigate the extensive margin. The benefit of this model type is that we can interpret the regression coefficients as percentage point changes in the conditional average probability to invest. In the robustness checks (see Table 3.5), we also calculate logit regressions that confirm our baseline analyses. As our main variables of interest are dummy variables and the dependent variable in the intensive margin models is the logarithm of investment, our regression coefficients are roughly equal to the DAL-induced relative changes of the investment activity. To get an unbiased estimate of the relative change, we apply the formula of Kennedy et al. (1981) and calculate the relative change as $exp\left[\hat{\beta}_i - \frac{1}{2} \cdot Var(\hat{\beta}_i)\right] - 1$.

Table 3.3 presents the regression results. Models (1) to (4) refer to investment at the extensive margin, while the models (5) to (8) contain results for the intensive margin. In the first specifications, we only use establishment fixed effects and year fixed effects to account for a potential over-control bias. In the other specifications, we gradually add industry-year fixed effects, district controls and establishment controls. Our preferred specifications are the fully specified models (4) and (8). We cluster heteroscedasticity-robust standard errors at the firm level, since investment decisions are made by the firm rather than the establishment.[18]

The coefficient on DiD is positive and statistically significant in all models. Thus, we find robust empirical support for H1 suggesting that the DAL bonus depreciation increased investment activity either by new investment projects or by an anticipation of investment projects. In our preferred specification in Column (4), we estimate a treatment effect on the probability to invest of 3.23 percentage points. Compared to the average probability to invest in East establishments (85.68 percent, Table 3.2), this implies an

[18] In unreported robustness checks, we also calculate bootstrapped standard errors that are very close to the standard errors reported here. We report R squared as well as the adjusted R squared. Both R-squared measures account for the explanatory power of the establishment fixed effects.

increase of gross investments at the extensive margin by 3.8 percent (= 0.0323/0.8568). Without any controls in Column (1), we find a slightly larger effect size on investment at the extensive margin of 4.4 percent (= 0.0378/0.8508). The average increase in investment at the intensive margin can be calculated by applying the Kennedy et al. (1981) formula to the coefficient estimates in Columns (5) to (8). For the full model in Column (8), we find an increase of 11.7 percent $[0.117 = exp\,(0.111 - 1/2 \cdot 0.0199) - 1]$ and for the reduced model in Column (5) an increase of 14.8 percent $[0.148 = exp\,(0.138 - 1/2 \cdot 0.0195) - 1]$. Thus, the DAL increased the conditional volume of investment by about 11 to 15 percent. Combining both estimates, we calculate an aggregate increase in real gross investments in the manufacturing sector ranging from 16.0 percent ($= 1.1038 \times 1.117$) to 19.9 percent ($= 1.1038 \times 1.148$).

3.5.2. Asset Structure

In the models (1) to (6) of Table 3.4, we perform similar analyses as in Table 3.3, but use equipment investments, building investments, and land investments at the extensive and intensive margins as dependent variables (H2). In these models, we use the specification with all control variables of Eq. 3.3. In Appendix F, we also perform alternative regressions without control variables and obtain qualitatively and quantitatively robust results. Our evidence provides strong empirical support for H2 suggesting a stronger impact of the DAL on investment goods with long regular depreciation periods. Furthermore, we only find relatively moderate effects for equipment investments that can be depreciated over a small number of periods (Devereux et al. 2009 assume an average period of 7 years for such investment goods) and also by the declining balance method that reduces the disadvantage of the regular depreciation scheme in relation to bonus depreciation.

Using similar calculations as in Section 3.5.1 (i.e., comparisons with Table 3.2 and the Kennedy et al. 1981 formula), the coefficients in Table 3.4 translate into an abnormal increase in equipment investment at the extensive margin of 3.8 percent and 3.4 percent at the intensive margin, resulting in an aggregate effect on equipment of 7.3 percent. This relatively moderate effect is contrasted by an abnormal increase in building investment at the extensive margin of 28.6 percent and at the intensive margin of 37.3 percent, resulting in an aggregate increase in building investment activity by 76.6 percent. Land investment increased at the extensive margin by 37.6 percent and at the intensive margin by 51.2 percent, which implies an overall increase in land investment of 108.0 percent. While these large investment responses do not allow for definitive statements whether the DAL bonus depreciation resulted in additional investment projects or rather a temporal anticipation of investment activity for that type of capital goods, we can provide clear evidence

Table 3.3: Gross investment at the extensive and intensive margin

Variables	Extensive margin				Intensive margin			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DiD	**0.0378*** **	**0.0356*** **	**0.0320*** **	**0.0323*** **	**0.1380*** **	**0.1190*** **	**0.1070*** **	**0.1110*** **
	(0.0034)	**(0.0034)**	**(0.0036)**	**(0.0036)**	**(0.0195)**	**(0.0192)**	**(0.0201)**	**(0.0199)**
Capital stock				0.0088***				0.1310***
				(0.0001)				(0.0050)
Revenue per capital				-0.0302*				0.3230*
				(0.0181)				(0.1720)
Unemployment rate			-0.0021***	-0.0020***			-0.0131***	-0.0116***
			(0.0006)	(0.0006)			(0.0033)	(0.0033)
GDP per capita			-0.0077	-0.0091			0.1740***	0.1530**
			(0.0092)	(0.0092)			(0.0630)	(0.0625)
Population			-0.0164**	-0.0158**			-0.0157	-0.0099
			(0.0074)	(0.0074)			(0.0397)	(0.0393)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry–year FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	544,085	544,085	544,085	544,085	470,548	470,548	470,548	470,548
Establishments	68,289	68,289	68,289	68,289	63,733	63,733	63,733	63,733
R-squared	0.472	0.472	0.472	0.473	0.702	0.704	0.704	0.705
Adjusted R-squared	0.396	0.396	0.397	0.397	0.655	0.657	0.657	0.658

Note: OLS regressions with establishment fixed effects and clustered standard errors at the firm level (in parentheses). In models (1)-(4), the dependent variable is a dummy variable with a value of one for an establishment i with positive gross investments in t (extensive margin). In models (5)-(8), the dependent variable is the logarithm of positive gross investments of establishment i in t (intensive margin). DiD is an interaction term of a dummy variable for establishments in Eastern German states and a dummy variable for the DAL treatment period (1995–1998). Capital stock is the logarithm of the capital stock of establishment i and Revenue per capital is the ratio of sales revenue to the capital stock. Unemployment rate is the unemployment rate of the district of establishment i in t in percentage points. GDP per capita (Population) is the logarithm of the gross domestic product per capita (the number of inhabitants) of this district. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

Table 3.4: Investment goods at the extensive and intensive margin

Variables	Equipment		Buildings		Land		Share of building and land
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive	Intensive
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
DiD	**0.0322*** **	**0.0336***	**0.0813*** **	**0.3180*** **	**0.0192*** **	**0.4350** **	**0.0377*** **
	(0.0037)	**(0.0189)**	**(0.0054)**	**(0.0498)**	**(0.0027)**	**(0.2080)**	**(0.0028)**
Capital stock	0.0088***	0.1380***	0.0112***	-0.0846***	0.0012***	-0.0242	-0.0023***
	(0.0009)	(0.0049)	(0.0009)	(0.0148)	(0.0004)	(0.0626)	(0.0005)
Revenue per capital	-0.0303*	0.3470**	0.0044	-0.3760***	0.0001	7.3590	-0.0126***
	(0.0181)	(0.1660)	(0.0074)	(0.0337)	(0.0001)	(67.200)	(0.0035)
Unemployment rate	-0.0019***	-0.0104***	-0.0006	-0.0396***	-0.0004	-0.0274	-0.0001*
	(0.0006)	(0.0032)	(0.0008)	(0.0095)	(0.0004)	(0.0417)	(0.0004)
GDP per capita	-0.0097	0.1520**	-0.0201*	0.2560*	-0.0078	0.1610	-0.00461
	(0.0094)	(0.0606)	(0.0119)	(0.1490)	(0.0053)	(0.6200)	(0.0062)
Population	-0.0175**	-0.0010	-0.0094	0.0359	-0.0039	-0.3070	0.0028
	(0.0052)	(0.0264)	(0.0030)	(0.0895)	(0.0264)	(0.2900)	(0.0035)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry–year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	544,085	468,458	544,085	114,285	544,085	18,175	470,228
Establishments	68,289	63,616	68,289	30,828	68,289	9,716	63,640
R-squared	0.471	0.711	0.450	0.489	0.274	0.623	0.288
Adjusted R-squared	0.394	0.666	0.371	0.298	0.170	0.170	0.176

Note: OLS regressions with establishment fixed effects and clustered standard errors at the firm level (in parentheses). For equipment (building, land) investment at the extensive margin in Model (1) (3, 5), the dependent variable is a dummy variable with a value of one for an establishment i with positive equipment (building, land) investments in t. For equipment (building, land) investment at the intensive margin in Model (2) (4, 6), the dependent variable is the logarithm of positive equipment (building, land) investments of establishment i in t. In Model (7), the dependent variable is the natural logarithm of the ratio of building plus land investments to total investments. DiD is an interaction term of a dummy variable for establishments in Eastern German states and a dummy variable for the DAL treatment period (1995–1998). Capital stock is the logarithm of the capital stock of establishment i and Revenue per capital is the ratio of sales revenue to the capital stock. Unemployment rate is the unemployment rate of the district of establishment i in t in percentage points. GDP per capita (Population) is the logarithm of the gross domestic product per capita (the number of inhabitants) of this district. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

that the DAL bonus depreciation did not only increase aggregate gross investment but also affected the asset structure and resulted in a large growth of investments in structures and land.

This is also underlined by Column (7) of Table 3.4, where the building and land investment share (i.e., the ratio of building and land investment to total investment) is the dependent variable. The fraction of building and land investments increased by 3.8 percentage points. Compared to an average building and land investment share of 15.9 percent (own calculations using Table 3.2), this implies a relative increase in the fraction of building and land investment activity by 23.8 percent.[19]

3.5.3. Robustness Checks

To confirm the robustness of our main findings, Table 3.5 reports additional estimates for gross investment (Columns 1-2), equipment investment (Columns 3-4), building investment (Columns 5-6), and land investment (Columns 7-8) when performing 4 alternative specifications. A potential concern might be that the DAL bonus depreciation program did not only affect investment activities of existing firms, but also resulted in the foundation of new establishments in East Germany or the relocation of establishments from the West to the East. In this case, our estimates in Section 3.5.1 would also capture the location decision of establishments, which could result in an overestimation of the pure investment response. Therefore, we perform an additional test that restricts our sample to establishments that existed in 1995 in both parts of Germany in Panel A. While this reduces our observation number to 241,147, it does not largely affect our results. Indeed, we still find statistically significant investment responses for gross investments at the extensive margin and for all investment types. If we use the investment responses of the different asset classes (equipment, buildings, land) to calculate an aggregate investment response,[20] we find an increase in gross investment activity of 10.5 percent, which is somewhat smaller than our baseline estimate of 16.0 percent. Thus, part of the investment response might fall on (re)location decisions as a consequence of the DAL program.

Another concern might be that our treatment and control groups might differ from each other with regard to technology access. To account for that issue, we restrict our analysis to a sample of West German firms with establishments in both parts of Germany (i.e., at least one establishment in the East and one establishment in the West).

[19]Goolsbee (1998) argues that investment tax incentives increase asset prices, which dampens their impact on real investment. For our purposes, this should not be a problem, since we deflate the value of building investment at the intensive margin in all specifications by a regional building price index. In additional analyses, we only find weak evidence for a relevant impact of the DAL on building prices (see Appendix C).

[20]We calculate the investment response for each asset class and then weight each of theses responses with the average fraction of investments in the asset type to overall investments. In doing so, we only consider statistically significant coefficients.

Table 3.5: Robustness Checks

Investment type	Gross investment		Equipment		Buildings		Land	
Investment margin	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Balanced panel								
DiD	**0.0210*** (0.0043)**	**0.0334 (0.0253)**	**0.0215*** (0.0044)**	**-0.0213 (0.0240)**	**0.0478*** (0.0075)**	**0.254*** (0.0627)**	**0.0212*** (0.0040)**	**0.4810* (0.2580)**
Observations	241,147	219,744	241,147	218,899	241,147	63,744	241,147	10,396
Establishments	24,336	24,079	24,336	24,069	24,336	15,487	24,336	5,375
R-squared	0.446	0.696	0.442	0.704	0.463	0.474	0.290	0.616
Adjusted R-squared	0.383	0.658	0.379	0.668	0.402	0.303	0.210	0.181
All models include establishment and country controls, establishment fixed effects, year fixed effects and industry-year fixed effects.								
Panel B: Mixed groups with parent fixed effects								
DiD	**0.0127 (0.0144)**	**0.3180*** (0.0892)**	**0.0101 (0.0144)**	**0.2300*** (0.0881)**	**0.0711*** (0.0169)**	**0.3140* (0.1790)**	**0.0125 (0.0087)**	**0.5660 (0.7240)**
Observations	38,259	31,389	38,259	31,188	38,259	8,898	38,259	1,075
Establishments	7,308	5,996	7,308	5,995	7,308	2,627	7,308	561
R-squared	0.503	0.811	0.505	0.815	0.533	0.517	0.382	0.521
Adjusted R-squared	0.405	0.771	0.408	0.776	0.442	0.359	0.262	0.146
All models include establishment and country controls, establishment, year, parent, and industry-year fixed effects.								
Panel C: Standard errors clustered at industry-year-level								
DiD	**0.0323*** (0.0042)**	**0.1110** (0.0437)**	**0.0322*** (0.0042)**	**0.0336 (0.0428)**	**0.0813*** (0.0060)**	**0.318*** (0.0443)**	**0.0192*** (0.0023)**	**0.4350** (0.1710)**
Observations	544,805	470,548	544,805	468,458	544,805	114,285	544,805	18,175
Establishments	68,289	63,733	68,289	63,616	68,289	30,828	68,289	9,716
R-squared	0.473	0.705	0.471	0.711	0.450	0.489	0.274	0.623
Adjusted R-squared	0.397	0.658	0.394	0.666	0.371	0.298	0.170	0.170
All models include establishment and country controls, establishment fixed effects, year fixed effects and industry-year fixed effects.								
Panel D: Logistic models								
DiD	**0.3170*** (0.0350)**		**0.2890*** (0.0342)**		**0.4960*** (0.0239)**		**0.3400*** (0.0410)**	
Observations	544,805		544,805		544,805		544,805	
Establishments	68,289		68,289		68,289		68,289	
Log likelihood	-167,820		-171,565		-212,211		-66,652	
All models include establishment and country controls, establishment fixed effects, year fixed effects and industry-year fixed effects.								

Note: This table reports robustnest checks for the analyses in Tables 3.3 and 3.4. Panel A uses a balanced panel of firms, Panel B performs the baseline regression for mixed firm groups, while accounting for parent fixed effects, Panel C repeats the baseline specification with standard errors clustered at the industry-year level, and Panel D contains the results of estimating logistic regressions for the extensive margin of investment. DiD is an interaction term of a dummy variable for establishments in Eastern German states and a dummy variable for the DAL treatment period (1995–1998). ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

We also add parent fixed effects to account for the fact that specific establishments belong to the same firm. This approach ensures that West and East establishments belong to the same firms, have access to the same technologies and are widely similar to each other. In Panel B, we reestimate Eq. (3.3) for this largely reduced panel with only 38,259 observations. Again, we find significant investment responses for gross investments, equipment investments and building investments. For land investments, the coefficient estimates are positive but not significantly different from zero, which is likely due to the low number of observations with positive land investments (only 1,075 firm-year observations). If we use the investment responses of the different asset classes (equipment, buildings, land) to calculate an aggregate investment response, we find an increase in gross investment activity of 32.5 percent, which is larger than our baseline estimate. This has two implications. First, potential differences in technology access between the treatment and control group in our baseline analysis should not result in an overestimation, but could rather bias our baseline estimate downwards. Second, firms with establishments in both parts of Germany could react more strongly due to subsidy shopping opportunities. In Appendix D.4, we document the robustness of this finding by adding parent-year fixed effects to the specification in Panel B. An addition, we also show that our baseline results are qualitatively and quantitatively robust to an alternative control group based on a pre-matched control group.

In Panel C and Panel D, we account for two potential technical concerns. In Panel C, we cluster standard errors at the industry-year level and not at the firm level. This does not significantly affect our results. In Panel D, we perform logistic regressions instead of linear probability models for the extensive margin analysis. In these logistic models, we still find statistically significant evidence that the DAL program increased real investment activity for gross investments, equipment, buildings and land. Overall, Table 3.5 documents strong robustness of our baseline regression results.

3.5.4. Tax Planning and Accounting Incentives

For the firm heterogeneity tests (H3a and H3b), we perform regressions with triple interaction terms as in Eq. (3.4). Similar to the last section, we account for all control variables. A challenge for our analysis is that information on legal structure (*Corp* dummy) and the activity of the firm's business owner (*Owner* dummy) relies on the Cost Structure Survey. Different from the Investment Survey, the Cost Structure Survey is not provided for the universe of German manufacturing firms but only for a sample of typically larger firms. This provides us with two alternative approaches. First, we can restrict our heterogeneity tests to this smaller sample of 243,919 observations of 45,381 establishments. However, a disadvantage of this approach is that this smaller sample might not be representative for the universe of all establishments.

Table 3.6: Firm heterogeneity: gross investment at the extensive and intensive margin

Variables	Extensive margin				Intensive margin			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DiD	0.0284***	0.0294***	0.0264***	0.0108	0.0587***	0.2240**	0.0601***	0.1040
	(0.0039)	(0.0113)	(0.0044)	(0.0120)	(0.0212)	(0.0891)	(0.0233)	(0.0917)
DiD Large	**-0.0152**		**-0.0164**	**0.0091**	**0.1780*** **		**0.1810*** **	**0.2580*** **
	(0.0101)		**(0.0103)**	**(0.0124)**	**(0.0577)**		**(0.0585)**	**(0.0774)**
DiD Group	**0.0177**		**0.0154**	**0.0314** **	**0.1330** **		**0.1130* **	**0.1010**
	(0.0108)		**(0.0109)**	**(0.0152)**	**(0.0588)**		**(0.0585)**	**(0.0891)**
DiD Owner		**-0.0067**	**0.0332** **	**-0.0058**		**0.1450**	**0.0569**	**0.1620**
		(0.0198)	**(0.0155)**	**(0.0199)**		**(0.135)**	**(0.0993)**	**(0.134)**
DiD Corp		**0.0007**	**0.0231** **	**0.0058**		**-0.1510**	**-0.1440* **	**-0.1140**
		(0.0124)	**(0.0105)**	**(0.0126)**		**(0.0941)**	**(0.0734)**	**(0.0938)**
Large	0.0053		-0.0073	-0.0312***	0.3380***		0.3050***	0.3710***
	(0.0049)		(0.0050)	(0.0063)	(0.0264)		(0.0276)	(0.0358)
Group	-0.0332***		-0.0339***	-0.0334***	-0.1750***		-0.1750***	-0.2210***
	(0.0045)		(0.0045)	(0.0057)	(0.0227)		(0.0227)	(0.0322)
Owner		-0.0165**	-0.0134***	-0.0191**		0.1330***	0.1360***	0.1270***
		(0.0078)	(0.0059)	(0.0079)		(0.0454)	(0.0409)	(0.0459)
Corp		-0.0123*	0.0061	-0.0134**		0.1110***	0.1340***	0.1050***
		(0.0068)	(0.0044)	(0.0068)		(0.0406)	(0.0344)	(0.0406)
Establishment controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry–year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	544,085	243,919	544,085	243,919	470,548	219,323	470,548	219,323
Establishments	68,289	45,381	68,289	45,381	63,733	42,895	63,733	42,895
R-squared	0.474	0.543	0.475	0.544	0.705	0.778	0.779	0.779
Adjusted R-squared	0.398	0.438	0.399	0.439	0.659	0.724	0.725	0.725

Note: OLS regressions with establishment fixed effects and clustered standard errors at the firm level (in parentheses). In models (1)-(4), the dependent variable is a dummy variable with a value of one for an establishment i with positive gross investments in t (extensive margin). In models (5)-(8), the dependent variable is the logarithm of positive gross investments of establishment i in t (intensive margin). DiD is an interaction term of a dummy variable for establishments in the Eastern German states and a dummy variable for the DAL treatment period (1995–1998). DiD Large is an interaction term of a dummy variable for firms with at least 250 staff members (Large) and DiD. DiD Group is an interaction term of a dummy variable for firms with more than one establishment (Group) and DiD. DiD Owner-Managed is an interaction term of a dummy variable for firms that are managed by an owner (Owner-managed) and DiD. DiD Corporation is an interaction term of a dummy variable for corporations with limited liability (Corporation) and DiD. All models include the full control variable setting, as well as year and industry-year fixed effects. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

Second, we can perform the analysis for the full sample, while setting the value of the dummy variables *Owner* and *Corp* to one only for firms that provide the corresponding information. A disadvantage of this approach is that it increases white noise in our data, since a value of zero for *Corp* could either mean that the firm is not a corporation or that such information is not available. To account for both challenges, we perform both approaches and report results in Table 3.6. In this table, we also perform additional tests with a reduced number of triple difference interaction terms in the regressions.

Most notably, we find evidence for H3a that expects a stronger investment reaction for multi-establishment firms due to opportunities for subsidy shopping, as indicated by the positive and statistically significant interaction term *DiDLarge* at the intensive margin. Thus, firms with more than one establishment had a stronger investment reaction compared to single-establishment firms. We find an additional DAL-driven increase in investment activity for large firms ranging from 19.6 percent (full sample) to 29.0 percent (restricted sample).[21]

Regarding multi-establishment firms, we find evidence for a significantly higher investment response in most specifications. While we find a statistically significant effect on investment at the extensive margin for the full sample in Model (4), we obtain a statistically significant increase on investment at the intensive margin in Models (5) and (7). Overall, our evidence suggests that large firms and also firms with multiple establishments reacted more strongly to the investment incentives of the German DAL program. In line with H3a, this suggests that lower costs of tax planning or higher opportunity for subsidy shopping are positively associated with the effectiveness of investment tax incentives.

By contrast, Table 3.6 does not provide empirical support for H3b. Overall, there is only one specification with an statistically significant and abnormally high investment response of owner-managed firms in model (3). For *DiDCorp* we find an abnormally high investment response for the full sample at the extensive margin (model 3), but an abnormally low investment response for investment at the intensive margin (model 7). Thus, the overall evidence is mixed and the results regarding H3b are inconclusive.

3.5.5. Additional Tests and Analyses

As documented in detail in Appendices D.1 to D.5, we perform the following additional tests and analyses. In Appendix D.1, we analyze the effect of the German DAL on building prices. Different from Goolsbee (1998), we find only weak pricing effects. Even if we allocate the full change in building price indices to the DAL, the DAL-induced increase in building prices is only 4.4 percent compared to an increase in real investment activity

[21] These aggregate effects result from an insignificant effect on investment at the extensive margin and a statistically significant effect on investment at the intensive margin from 19.6 percent (full sample) to 29.0 percent (restricted sample).

in buildings of 76.6 percent and in land of 108.0 percent.

To alleviate concerns regarding the appropriateness of our control group, we use propensity score matching to increase the similarity of the treatment and control group and re-estimate the regression Eq. (3.3) with the matched sample. We report results in Table D.3.2 in Appendix D.2 that qualitatively and quantitatively confirm our baseline evidence. In Table D.3.2, we find a range for the aggregate DAL effect on investment activity of 15.2 percent to 18.4 percent, which is very close to our baseline range of 16.0 percent ot 19.9 percent.

In Appendix D.3, we investigate whether firms anticipated the expiration of the DAL and therefore simply shifted investment decisions from the future into the DAL period. As shown in Table D.3.3, there is no evidence of a negative investment effect right after the DAL expired. Put differently, there was no abnormal decrease of investments in East establishments compared to the control establishments after the DAL period. This indicates that the main results are not driven by anticipation effects.

In Appendix D.4, we show that our results are robust to several alternative control variable settings and thus are not driven by our control variable specifications. In Table D.3.4, we report the results for different investment types (equipment, buildings, land) without regression control variables and industry-year fixed effects. In Table D.3.5, we account for a potential concern of endogenous regression controls and report results for gross investments and the investment types if we use once-lagged control variables. In Table D.3.6, we further include EBITDA per capital (as a measure of cash flow), interest per capital (as a measure of debt ratios), and the legal form of the firm (as a measure of financing opportunities), since one might argue that revenue per capital stock is not sufficient to control for capital constraints. This information is not available for all observations, causing our sample size to shrink by more than 50 percent. In Table D.3.7, we re-estimate our test for a panel of West firms with establishments in both parts of Germany, but further add parent-year fixed effects. By doing so, we identify the DAL treatment effect at the level of the establishment holding firm-years constant. Thus, for each firm, we compare investment responses between West and East establishments of the same firm. Average treatment effects remain very similar to Panel B of Table 3.5.

Lastly, considering the partially mixed evidence on firm heterogeneity in Table 3.6, we re-estimate this table in Appendix D.5 for different types of investment goods in the Tables D.3.8 (equipment), D.3.9 (building) and D.3.10 (land). These additional tests confirm our main findings of Table 3.6. Most relevant, we find abnormally high investment responses for large firms and multi-establishment firms in line with H3a. By contrast, we do not find consistent evidence for owner-managed firms and corporations.

3.6. Conclusion

Using high-quality data of the German Federal Statistical Office at the establishment level, we exploit a bonus depreciation regime (Development Area Law, DAL) for establishments in the East part of Germany to analyze whether, to what extent, and how such tax regimes affect *real* investment activity. We find strong empirical evidence that the DAL increased real investment activity by 15 to 20 percent. This moderate average effect masks a large heterogeneity in investment responses. With regard to asset types, we find an aggregate investment response of about 7 to 10 percent for equipment, 75 to 90 percent for buildings and 108 to 120 percent for land investments. These findings suggest that firms rationally react to investment tax incentives, and at least temporarily adjust their asset structure in order to optimize tax benefits. We further provide evidence that large firms and firms with more than one establishment have a higher investment response.

We also discuss potential limitations of our study. First, as our sample is limited to the manufacturing industry of Germany, results may not be representative to empirical settings in other industries and countries. Second, the observed policy variation took place at a time where German business tax rates were higher than today. Thus, bonus depreciation programs might be less effective if tax rates are lower. Nevertheless, our elasticity estimates are rather at the lower bound of other studies that analyze more recent bonus depreciation regimes. Therefore, our paper underlines the finding of several empirical studies that bonus depreciation seems to be a very effective strategy to increase investment activity, and that *real* investments are affected. That holds especially for asset classes with long regular depreciation periods.

A policy implication of our paper is that bonus depreciation regimes will be more effective if standard depreciation periods are long. Thus, reducing standard depreciation periods in the long run might promote investment activity, but also will make temporal and anti-cyclical investment tax incentive programs less effective. A second policy implication is that large firms and firms with more opportunities for subsidy shopping receive higher benefits from investment tax incentive programs. Thus, governments should be careful and restrict subsidies that are given to such firms, as investment tax incentives might reduce average investment quality (Eichfelder, Jacob, and Schneider 2023), and could harm competition between small and large firms. Lastly, accounting incentives, as highlighted by Edgerton (2012), do not seem to play a major role for the effectiveness of bonus depreciation policies.

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Appendices

Appendix A: Present Value of DAL Benefits

A.1 Calculation of DAL Present Value

We calculate the DAL present value as the sum of the tax savings from bonus depreciation during the first year plus the present value of remaining depreciations in the future, and minus the present value of the 'fastest' alternative depreciation scheme without bonus depreciation. Since we calculate the DAL benefit from the perspective of a given period t, we do not account for changes in taxes and the after-tax cost of funds in future periods (e.g., for the reduction of corporate income tax rates resulting from the German tax reform 1999/2000/2001). Taking into account that German tax rates declined after 1998, we calculate a lower-bound estimate of the ex-post DAL benefit, because the value of depreciation benefits increases with the tax rate. We also do not account for the possibility of future tax losses that would reduce the present value of future tax depreciation.[22] We assume that the DAL bonus depreciation is fully utilised in the first year and the investment is executed in the middle of the year. The present value of DAL benefits is then equal to

$$PV_t = \tau_t \cdot \Phi_t \cdot \left[\beta_t + (1-\beta_t) \cdot \sum_{t+x}^{t+X} \frac{\delta reg_{t+x}}{(1+\rho_{\tau t})^{x-1}} - \sum_{t+x}^{t+X} \frac{\delta alt_{t+x}}{(1+\rho_{\tau t})^{x-1}} \right], \tag{A.3.1}$$

where Φ_t denotes the total sum of DAL-funded depreciations in the East German states in a given year t, τ_t the average tax rate in t, $\rho_{\tau t}$ the after-tax cost of funds in t, and x is an index for following depreciation years. The bonus depreciation rate of a given period (ranging from 40 percent to 50 percent) is denoted β_t. Thus, $(1-\beta_t)$ is the remaining book value that has to be depreciated by the regular scheme. The parameters $0 \leq \delta reg_{t+x} \leq 1$ and $0 \leq \delta alt_{t+x} \leq 1$ describe the allocation of depreciations under the regular scheme and the alternative scheme, respectively.

German tax instalments are affected by depreciation allowances. In line with Cohen, Hansen, and Hassett (2002), we therefore reduce the number of discounting periods x by one. Subsidy reports of the German Federal Government (German Federal Government, 1995–1999; German Federal Ministry of Finance, 2001–2010) do not report the total amount of depreciations Φ_t, but only the amount of tax losses resulting from bonus depreciation $\tau_t \cdot \Phi_t \cdot \beta_t$. We use this information to calculate $\tau_t \cdot \Phi_t$.

[22] In case of a tax loss, the remaining depreciation volume does not result in a reduction of the tax burden (the tax payment is zero anyway), but increases the loss carryforward. Thus, future losses might decrease the present value of the remaining depreciation volume.

A.2 After-Tax Coft of Funds

An important aspect affecting the present value of depreciation allowances is the after-tax cost of funds. Using the definition of weighted average costs of capital (WACC) (Hulse and Livingstone, 2010; Frank and Shen, 2016), after-tax costs of funds in a given period t can be written as

$$\rho_{\tau t} = \rho_{dt} \cdot d \cdot (1 - \tau_{t^*}) + \rho_{et} \cdot (1 - d). \tag{A.3.2}$$

In Eq. A.3.2, ρ_{dt} denotes the pre-tax cost of debt capital at time t, ρ_{et} the cost of equity capital, and d the – for simplicity, this is a constant – fraction of debt capital. The tax deductibility of interest payments at the firm level is included in the tax rate τ_{t^*}, thereby accounting for the limited tax-deductibility of interests for long-term debt with regard to the German local business tax (German: *Gewerbesteuer*).

To approximate the cost of debt capital ρ_{dt} for a given year t, we use average long-term interest rates published by the German Central Bank (German: *Deutsche Bundesbank*).[23] This can be justified by the fact that investments are generally financed by equity and long-term debt, while short-term debt is more relevant for operational business. The average interest rate between 1995 and 2008 was 6.07 percent. The average fraction of debt capital is taken from representative balance sheet statistics of the manufacturing industry, which are also provided by the German Central Bank (2001–2012). In line with Hulse and Livingstone (2010), we focus on the ratio of long-term debt to equity. Therefore, we assume that short-term debt and accruals result from operational business and do not affect the after-tax cost of funds of long-term business investment. We do not observe strong changes in d during our sample period. Therefore, we use a fixed average value of $d = 0.3439$.

In contrast to the cost of long-term debt, the cost of equity is not published by the German Central Bank. While there are a number of proxies for ρ_{et}, there is no generally accepted definition of this variable. Botosan, Plumlee, and Wen (2011) describe 10 alternative proxies with a positive and significant correlation with future realised returns; we rely on the mean of these 10 proxies for equity cost as reported by Botosan, Plumlee, and Wen (2011). It should be noted that the values of ρ_{et} are based on U.S. data instead

[23]Since the definitions of reported interest rates of the German Central Bank change over time, we rely on a number of different proxies for the cost of debt capital. For 1997 to 2002, we use average interest rates for business credits ranging from € 500,000 to € 5 million (BBK01.SU0509). For 2003 and thereafter, we use interest rates for credits to corporations exceeding € 1 million and a duration of more than 5 years (BBK01.SUD129). For the period from 1991 to 1996, we use floating long term mortgage interest rates (BBK01.SU0049) as business interest rates are not available. We use 'overlapping' periods with more than one possible proxy of interest rates to adjust all interest rates to a consistent definition over the whole period, using interest rates from 1997 to 2002 as our reference point (BBK01.SU0509).

of German data. However, this should not be a severe problem, as Hail and Leuz (2006) do not find evidence for strong differences in the equity cost of capital in both countries. Nevertheless, we re-weight the equity cost by the corresponding differences reported in Hail and Leuz (2006) and obtain an average cost of 11.0 percent. To obtain average values per year, we relate this value to the average cost of long-term debt and obtain a ratio of 1.814. Thus, average ρ_{et} can be approximated by $\rho_{dt} \cdot 1.814$. This is very close to the relationship between ρ_{et} and ρ_{dt} of 1.8 as assumed by Hulse and Livingstone (2010).

The tax rate τ_{t^*} is a weighted effective tax rate with respect to the deduction of interest expenses of business establishments in East Germany. To calculate τ_{t^*}, we account for the distribution between profits generated by corporations (taxed at corporate income tax rates) and by self-employed businesses and partnerships (taxed at personal income tax rates). The fraction of profits generated by corporations is approximated by the corresponding distribution of revenue as documented in the VAT statistics of the German Federal Statistical Office. To calculate the effective tax rate of corporations (partnerships), we use the corporate income tax rate (the maximum marginal personal income tax rate) for accumulated business profits of a given year. We further consider the solidarity tax surcharge (German: *Solidaritätszuschlag*) and the average local business tax rate (German: *Gewerbesteuer*) of the East German states. We account for the fact that local business tax payments were deductible from taxable income until 2007. In addition, we account for the local business tax credit for partnerships (German: *Gewerbesteueranrechnung*) and the add-backs of long-term debt for the German local business tax (German: *Hinzurechnungen*).

A.3 Depreciation Regulations

To calculate the parameters $0 \leq \delta reg_{t+x} \leq 1$ and $0 \leq \delta alt_{t+x} \leq 1$ in Eq. A.3.1, we rely on the depreciation regulations of the German income tax code for different asset classes as well as the distribution of real investments between buildings and equipment for a given year (see Tables 2.1, 3.1 and 4.1 in German Federal Statistical Office, 2010).

In the 1990s, new buildings were generally depreciated over a period of 25 years. For investments after 2000, these depreciation periods for new business buildings were increased to 33.3 years. For the modernisation (extension, improvement) of old buildings, longer depreciation periods of 40 and 50 years (depending on the construction date of the building) were in force. We use the average fraction of new buildings as reported in the German building statistics of the corresponding period to construct weights for old and new buildings. For modernisations, we assume that one half of investments are depreciated over 40 and the other half over 50 years.[24]

[24]We rely on the number of constructed non-residential buildings in the former East (new buildings versus modernisations during the period from 1993 to 1999, which was relevant for the Development Area

Furthermore, we account for declining depreciation schemes for new buildings and modernisations as documented in § 7 Section 5 of the German income tax code. These alternative depreciation schemes were available if (1) the new building was constructed (or an old building was modernised) with a building application before January 1994 or January 1995, respectively, or (2) the building was purchased before January 1994 or January 1995, respectively. As buildings are constructed after the building application has been submitted, we assume that declining depreciation schemes are available for two years after the expiration date (100 percent in the first year after the abolition and 50 percent in the second year).

There is no data on average tax depreciation periods for equipment investment in Germany. Therefore, we assume an average depreciation period of seven years as documented by Devereux et al. (2009). In the 1990s, the depreciation rate of the declining balance method for movable assets was 30 percent. Hence, we assume that all equipment investments use the declining balance method, as long as corresponding depreciations are 'higher' than the alternative linear depreciations. Note that the declining balance method was not available if the bonus depreciation was utilised. Therefore, the consideration of these programs reduces the relative benefit of bonus depreciation to some extent.

Appendix B: Calculation of Capital Stocks

Our calculation is based on Wagner (2010), who uses depreciation values for tax purposes reported in the Cost Structure Survey, information on the composition of investments from the Investment Survey and average depreciation periods for different asset classes (buildings and equipment) to compute capital stocks. Our method extends this approach in a number of ways and can be described by

$$K_{i,t-1} = \left(D_{it} \cdot \left(\alpha_{it}^{E} \cdot P_{t}^{E} + \alpha_{it}^{B} \cdot P_{t}^{B}\right) - I_{it}^{N}\right) \cdot \frac{1}{2}, \tag{B.3.1}$$

where $K_{i,t-1}$ is the capital stock at the end of the previous period (or beginning of the current period) of the firm i, D_{it} is the depreciation of i in t, α_{it}^{E} is the fraction of equipment investment of a given year, α_{it}^{B} the fraction of building investment in that year, and P_{t}^{E} $\left(P_{t}^{B}\right)$ the average depreciation period for equipment (building) investment in Germany in t.

Multiplying the sum of depreciations with the average depreciation period yields the investment value at the beginning of the operating period. To account for depreciations after the beginning of the operating period of an asset, we divide this value by two. Therefore, we assume that the average operating period has expired by a factor of 50%

Law).

for each asset. This implies further that price-adjusted depreciations are approximately evenly distributed over time. Note that investments in t have a positive effect on D_{it}. If investments are executed in the middle of the year, D_{it} should rather be a measure of the capital stock in the middle of the period instead of the beginning of the period. To account for that aspect, we deduct 50% of net investments I_{it}^{N} (defined as gross investment minus disinvestment) of firm i in time t.

The depreciation period P_t^E for equipment is assumed to be 7 years (see Devereux et al. 2009). For new buildings, the regular periods are 25 years (for old buildings 40 to 50 years). For simplicity, we do not account for declining depreciation schemes for buildings. This can be justified by the fact that declining schemes increase the present values of depreciation allowances, but not the average depreciation over the depreciation period. The composition of different asset classes is estimated by the distribution of investments α_{it}^E and α_{it}^B of the manufacturing industry in our data, with $\alpha_{it}^E + \alpha_{it}^B = 1$. To account for measurement error, we calculate average values for α_{it}^E and α_{it}^B by year, industry, business size (large firms compared to small firms with up to 250 staff members) and region (East versus West Germany).

The tax depreciation period for new buildings increased to 33.3 years in 2001, while depreciation periods for modernization remained unchanged. The increased depreciation period is only relevant for new installments. Thus, considering economic growth and declining depreciation schemes of preceding periods, we assume a declining adaptation process of the average depreciation period per firm over 25 years with $D_{2000+x} = D_{2000} + \Delta \cdot \sqrt{\frac{x}{25}}$, where D_{2000} denotes the average depreciation period in 2000 (29 years on average for old and new buildings), x the number of years after 2000 and the increase in the average depreciation period resulting from the reform. This yields an average depreciation period for buildings of 35.66 years in 2008.

The computation of capital stock may be affected by measurement error in D_t. This is especially a problem for a high variation of tax depreciations over time, implying a fluctuating capital stock. To account for that, we rely on estimated capital stocks of future periods to obtain a more consistent estimate of the capital stock of preceding periods. Hence, we define the capital stock of the preceding period as the capital stock of the following period plus investments and minus depreciations and disinvestments in t. In addition to fixed assets, and extending Wagner (2010), we consider leased investments as increasing the effective capital-in-kind. We rely on data from the Investment Survey to compute the ratio of leased assets to fixed assets by year, industry, business size and region (West versus East Germany). The value of fixed assets of each firm is multiplied by one plus the computed ratio.

A drawback of our data is that depreciation volumes of the Cost Structure Survey are only available at the firm level. Therefore, we allocate depreciations to the establishment.

We compute the ratio of the capital stock to the number of staff members by year, industry, business size (large firms compared to small firms with up to 250 staff members) and region (establishments in the West and establishments in the East). Using these ratios, we allocate the firms' capital stock to the establishments.

Appendix C: Calculation of the relative tax burden and elasticities

C.1 Effective Net-of-Tax Rate

The effective net-of-tax rate of investments in East German establishments can be written as

$$NETR_t = \frac{1 - \tau_t^W \cdot Z_t^W - s_t^W}{1 - \tau_t^E \cdot Z_t^E - s_t^E} \cdot \frac{1 - \tau_t^E}{1 - \tau_t^W}, \tag{C.3.1}$$

where τ_t^E, Z_t^E, s_t^E (τ_t^W, Z_t^W, s_t^W) denote the tax rate on profits, the present value of depreciations per € invested, and the effective ISL subsidy rate for East (West) Germany in a given period, respectively. As introduced in Appendix A, τ_t^E and τ_t^W are based on weighted tax rates of partnerships and corporations including taxes on income, the solidarity tax surcharge, and the local business tax. To calculate τ_t^E (τ_t^W), we use average local business tax multipliers (German: Hebesätze) for the East (West) German states. Z_t^E and Z_t^W are calculated as in Appendix A. The after-tax costs of funds of Appendix A.2 are used to compute discounted values of depreciation allowances.

We account for the fact that applications for ISL subsidies are generally related to investments of the preceding year. Therefore, the effective subsidy rate s_t^E is defined as the nominal ISL rate discounted by one period. As shown in Table 3.1, the ISL rate for equipment investment of large firms was 5% (small firms 10%) from 1995 to 1998, 10% (small firms 20%) in 1999, and 12.5% (small firms 25%) thereafter. Regarding building investment, funding rates were zero before 1999 and 10% (12.5%) for initial investments in 1999 (after 1999). In this case, there were no increased rates for firms with no more than 250 staff members. There was also a higher subsidy rate of 8% for equipment investments of all firms before 1997 if investments had been initiated before June 1994. We assume that this is relevant for 50% of investments in 1995 and 0% thereafter. Correspondingly, we assume that the increase of funding rates in 2000 (from 10.0% to 12.5%) was relevant for 50% of businesses in that period and for 100% thereafter. Note that s_t^W is zero, as ISL subsidies were restricted to investments in the East states. In addition, there was a funding gap for investments initiated before August 25, 1997 and completed after December 31, 1998. For simplicity, we do not consider this aspect in our calculations.

This can be justified by the fact that this funding gap was not expected by owners and managers.

C.2 Calculation of Investment Elasticities

Following Zwick and Mahon (2017), we calculate investment elasticities with respect to the net of the effective tax rate $1-\tau_\epsilon$ (in the following NETR), respectively the reciprocal of the tax term of the user cost of capital. For the calculation of the effective tax rate, we rely on the same assumptions as for the calculation of the relative tax burdens Appendix C.1) Consistent with our regression approach, we focus on the relative tax benefit of establishments in East Germany. Thus, we rule out any other tax law changes affecting establishments in both parts of Germany equally. We define the additional relative tax incentive of East German establishments during the treatment period as the difference between the average relative NETR in the treatment period and the average relative NETR in the post-treatment period. Thus, the change in the relative tax incentive of East German establishments is

$$\Delta NETR = \overline{NETR}_{TREAT} - \overline{NETR}_{POST}, \tag{C.3.2}$$

with $NETR_t = \frac{1-\tau_t^W \cdot Z_t^W - s_t^W}{1-\tau_t^E \cdot Z_t^E - s_t^E} \cdot \frac{1-\tau_t^E}{1-\tau_t^W}$. We calculate $\Delta NETR$ for large and small firms for three classes of buildings and equipment: 1a) initial building investment, 1b) new buildings (no initial investment), 1c) modernisation of buildings (no initial investment), 2a) initial equipment investment, 2b) non-initial equipment investment, and 2c) non-fundable equipment investment. To calculate the aggregate average $\Delta NETR$ for small and large firms, we make assumptions about the average distribution of these six different types of investments. Note that there are no official statistics on initial investments according to ISL or the fundability of assets.

As extensions of an establishment are considered as initial investment according to ISL, the majority of building constructions should be initial investments. Hence, in case of the aggregate volume of building investments, we assume that the majority (55%) are initial investments. Corresponding to the German building statistics, about 20% of building investments are modernizations. We regard the remainder (25% of building investments) as constructions, but not as initial investments (e.g. extensions of buildings, major enhancements of buildings). In case of equipment investments, we assume that the majority (55%) are replacement investments and are therefore not considered as initial investments. This is consistent with the empirical observation that the increased funding rates of the ISL 1999 did not result in a significant increase in aggregate ISL subsidy payments. As most equipment in the manufacturing sector was fundable (e.g. machines), we assume that non-fundable equipment is only 1/5 of the remaining equipment investments.

Thus, the remainder (4/5) falls on fundable initial equipment investments.

Appendix D: Additional Robustness Tests and Analyses

D.1 DAL and Building Prices

Goolsbee (1998) finds evidence that investment tax incentives increase asset prices, which dampens their impact on real investment. While studies on bonus depreciation do not provide support for such pricing effects (House and Shapiro 2008; Edgerton 2011; Zwick and Mahon 2017), we account for that by deflating building investment at the intensive margin by a regional building price index. Corresponding price indices are provided by the statistical offices of ten major federal German states. The state of Berlin has been excluded from our data. For the remaining five states (Bremen, Hamburg, Mecklenburg-West Pomerania, Rhineland-Palatinate, and Schleswig-Holstein), we rely on average building price indices for the West and the East German states. These average price indices are calculated using the average of existing GDP-weighted price indices for states in the former West (Baden-Württemberg, Bavaria, Hessen, Lower Saxony, North Rhine-Westphalia and Saarland) and in the former East (Brandenburg, Saxony, Saxony-Anhalt and Thuringia). Using building price indices from states in the East and the West, we calculate weighted building price indices for both parts of Germany and report the results in Table D.3.1.

Table D.3.1: Building price indices: manufactoring sector

Year	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008
East	97.9	98.1	97.3	96.3	95.0	94.7	94.1	94.3	95.3	97.8	100.0	102.5	110.4	114.6
West	93.9	94.2	93.9	94.5	94.7	95.9	96.7	96.9	97.2	98.4	100.0	102.3	109.6	113.6

Note: Weighted average building price indices for the manufacturing sector in the eastern and western German states. We calculate the indices from GDP-weighted price indices in the manufacturing sector as reported by the Statistical State Offices for the western German federal states (Baden-Württemberg, Bavaria, Hessen, Lower Saxony, North Rhine-Westphalia and Saarland) and the eastern states (Brandenburg, Saxony, Saxony-Anhalt and Thuringia). Due to the specific economic and legal conditions in Berlin, we do not account for price developments in that area. There are no corresponding price indices available for Bremen, Hamburg, Mecklenburg-West Pomerania, Rhineland-Palatinate or Schleswig-Holstein.

We find only weak pricing effects. Using 2005 as reference year, the building price index in East Germany in the treatment period (1995–1998) exceeds the index for West Germany by 3.0 percentage points on average, while it is slightly lower than the West German index in the post-treatment period (1.2 percentage points). Even if one were to assume that this change in the difference of the building price indices is exclusively the result of the DAL, this suggests a DAL treatment effect of 4.2 percentage points (estimated by the difference in the differences of 3.0 and minus 1.2) or 4.4 percent of the building price level in 1999. This is minor if compared to our estimated average response for real building investments of 83 percent that we calculate in Subsection 5.2.

D.2 Propensity Score Matching

To ensure the comparability and similarity of our treatment group and control group, we use propensity score matching (Caliendo and Kopeinig 2008). Relying on one-to-one matching with replacement and the base year 1999, we generate a pre-matched control group that is similar to our treatment group. We match on the following variables: investment activity (as measured by the logarithm of building investment and the logarithm of equipment investment; both increased by one to avoid undefined values), establishment size and economic activity (as measured by the logarithm of sales revenue and the logarithm of the number of employees), industry, firm type (single establishment firm, multi-establishment firm, multinational firm, establishment of a foreign firm), and the type of goods produced (input goods, investment goods, durables, commodities). We do not use the capital stock as a matching characteristic, as this variable has been constructed using the information on investments (for the calculation of the capital stock see Appendix B). We select 1999 as our base year for matching but also consider outcome values from future periods (2000–2008) for our time-variant matching variables to account for the common trends assumption. To ensure a minimum common support, we drop establishments with propensity scores that are higher than the maximum (and lower than the minimum) propensity score in our control group. We end up with a final sample of 89,734 observations from 7,406 establishments.

Table D.3.2 depicts the results from performing the baseline model (Table 3.3) for the matched sample. The coefficient estimates are in line with Table 3.3 qualitatively and quantitatively.

Table D.3.2: Gross investment at the extensive and intensive margin–Matched Sample

Variables	Extensive margin				Intensive margin			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DiD	0.0213***	0.0207***	0.0180***	0.0181***	0.1450***	0.1450***	0.1210***	0.1210***
	(0.0049)	(0.0049)	(0.0052)	(0.0052)	(0.0303)	(0.0299)	(0.0312)	(0.0309)
Capital stock				0.0048**				0.1230***
				(0.0019)				(0.0106)
Revenue per capital				-0.0162***				101.9
				(0.0011)				(86.66)
Unemployment rate			-0.0011	-0.0011			-0.0159***	-0.0148***
			(0.0001)	(0.0001)			(0.0055)	(0.0055)
GDP per capita			-0.0215	-0.0230			0.0866	0.0470
			(0.0166)	(0.0166)			(0.1010)	(0.0997)
Population			-0.0152	-0.0145			0.0496	0.0636
			(0.0105)	(0.0105)			(0.0680)	(0.0673)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry–year FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	89,734	89,734	89,734	89,734	81,777	81,777	81,777	81,777
Establishments	7,406	7,406	7,406	7,406	7,375	7,375	7,375	7,375
R-squared	0.371	0.373	0.373	0.374	0.669	0.673	0.673	0.675
Adjusted R-squared	0.314	0.315	0.315	0.316	0.636	0.640	0.640	0.641

Note: OLS regressions with establishment fixed effects and clustered standard errors at the establishment level (in parentheses). In models (1)-(4), the dependent variable is a dummy variable with a value of one for an establishment i with positive gross investments in t (extensive margin). In models (5)-(8), the dependent variable is the logarithm of positive gross investments of establishment i in t (intensive margin). DiD is an interaction term of a dummy variable for establishments in Eastern German states and a dummy variable for the DAL treatment period (1995–1998). Capital stock is the logarithm of capital stock of establishment i and Revenue per capital is the ratio of sales revenue to the capital stock. Unemployment rate is the unemployment rate of the district of establishment i in t in percentage points. GDP per capita (Population) is the logarithm of the gross domestic product per capita (the number of inhabitants) of this district. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

D.3 Investment Loopholes and Delays

As the DAL bonus depreciation provided a temporal enhancement in depreciation opportunities and the expiration of the program might have been a foreseeable event for firms, a potential reaction of firms could have been a temporal anticipation of investments that would have been executed otherwise in future periods. Thus, firms might anticipate investments from the period after the expiration (after 1998) of the program into the subsidy period (before 1998). If this is the case, it could lead to abnormal reduction of investment activity in treated East establishments compared to control establishments in West Germany. Therefore, we perform an additional test, considering not only the overall treatment effect in the period before the expiration (baseline model) but also a treatment effect in the post-DAL year 1999. We estimate the following model:

$$I_{it} = \beta_0 + \beta_1 \cdot DiD_{it} + \beta_2 \cdot PostDiD_{it} + \phi \cdot X_{it} + \alpha_i + \gamma_t + \theta_{it} + \epsilon_{it}. \tag{D.3.1}$$

In this model, $PostDiD_{it}$ is an interaction term of the post-DAL year 1999 and a dummy variable with a value of one for establishments in East German states. As documented by Table D.3.3, and opposite from expectations regarding an investment hole, we still find a positive regression coefficient for $PostDiD_{it}$. Thus and in line with our graphical evidence in Figure 3.2, we still find evidence for slightly higher investment activity in East establishments in the post-DAL year 1999. An explanation for our finding is the presence of temporal delays of investments that were initially intended to be executed within the DAL period. Considering that DAL-driven investment were especially relevant for buildings and project delays are a common problem in the building industry, it should not be surprising that not all DAL-related projects were finished in time before the funding period ended. Notwithstanding, Table D.3.3 does not provide evidence that investment projects were anticipated from the past-DAL period to maximise tax benefits before the DAL period ended.

Table D.3.3: Tests for post–DAL investment effects

Variables	Extensive margin				Intensive margin			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DiD	**0.0407*** **	**0.0383*** **	**0.0351*** **	**0.0356*** **	**0.1450*** **	**0.1240*** **	**0.1130*** **	**0.1210*** **
	(0.0036)	**(0.0036)**	**(0.0039)**	**(0.0039)**	**(0.0206)**	**(0.0204)**	**(0.0216)**	**(0.0213)**
Post DiD	**0.0184*** **	**0.0174*** **	**0.0163*** **	**0.0173*** **	**0.0405***	**0.0347**	**0.0337**	**0.0505** **
	(0.0043)	**(0.0043)**	**(0.0044)**	**(0.0044)**	**(0.0218)**	**(0.0218)**	**(0.0221)**	**(0.0220)**
Capital stock				0.0089***				0.1310***
				(0.0009)				(0.0050)
Revenue per capital				-0.0302*				0.3230*
				(0.0181)				(0.171)
Unemployment rate			-0.0019***	-0.0018***			-0.0127***	-0.0111***
			(0.0006)	(0.0006)			(0.0033)	(0.0033)
GDP per capita			-0.0052	-0.0064			0.1800***	0.1610**
			(0.0093)	(0.0093)			(0.0633)	(0.0628)
Population			-0.0174**	-0.0170**			-0.0180	-0.0132
			(0.0074)	(0.0074)			(0.0397)	(0.0393)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry–year FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	544,085	544,085	544,085	544,085	470,548	470,548	470,548	470,548
Establishments	68,289	68,289	68,289	68,289	63,733	63,733	63,733	63,733
R-squared	0.472	0.472	0.472	0.473	0.702	0.704	0.704	
Adjusted R-squared	0.396	0.397	0.397	0.397	0.655	0.657	0.657	

Note: OLS regressions with establishment fixed effects and clustered standard errors at the establishment level (in parentheses). In models (1)-(4), the dependent variable is a dummy variable with a value of one for an establishment i with positive gross investments in t (extensive margin). In models (5)-(8), the dependent variable is the logarithm of positive gross investments of establishment i in t (intensive margin). DiD is an interaction term of a dummy variable for establishments in Eastern German states and a dummy variable for the DAL treatment period (1995–1998). Post DiD is an interaction term of a dummy variable for establishments in Eastern German states and a dummy variable for the post DAL year 1999. Capital stock is the logarithm of capital stock of establishment i and Revenue per capital is the ratio of sales revenue to the capital stock. Unemployment rate is the unemployment rate of the district of establishment i in t in percentage points. GDP per capita (Population) is the logarithm of the gross domestic product per capita (the number of inhabitants) of this district. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

D.4 Alternative Control Variable Settings

The following tables report regression results for four alternative control variable settings to control if our results are driven by control variable choices. In Table 3.4, we investigate investment types including the full set of control variables. Table D.3.4 reports the same analysis without controls and industry-year fixed effects, similar to the simplest baseline analyses for gross investment (Table 3.3). The results confirm our baseline findings in Table 3.4. Like in table 3.3, we obtain quantitatively larger effects if we do not include regression controls and industry-year fixed effects. We find an increase of 10.5 percent (baseline 7.3 percent) for equipment, of 92.9 percent (baseline 76.6 percent) for building investment and of 121.3 percent (108.0 percent) for land investment.

A potential concern might be that our regression controls are endogenous with investment as dependent variable. Therefore, we estimate an alternative specification with once-lagged regression control variable and report results in Table D.3.5. As the choice of lagged controls reduces our observation period, the number of firm-year observation decreases to 468,549. If we use the investment responses of the different asset classes (equipment, buildings, land) to calculate an aggregate investment response (similar to Table 3.5), we obtain an overall investment response of 15.0 percent, which is very close to our baseline estimate.

Another concern might be an omitted variable bias. Therefore, we perform a robustness check that adds additional control variables at the level of the firm. These include the legal form of the company (corporation or pass-through entity), interest expenses per capital stock as a measure for liquidity and capital constraints and operating income (EBITDA) per capital stock as a measure for profitability. These variables are provided by the Cost Structure Survey and therefore only available for less than 50 percent of our sample. We report results in Table D.3.6. Using the investment responses of the different asset classes (equipment, buildings, land) to calculate an aggregate investment response (similar to Table 3.5), we obtain an overall investment response of 14.5 percent, which is very close to our baseline estimate.

Finally, we extend the robustness check of Panel B in Table 3.5 by adding parent-year fixed effects in Table D.3.7. In doing so, we control for year parent-year combination and thus use only the variation at the firm level to identify the DAL effect. Hence, holding firm-year combinations constant, we compare investment activities of East and West establishments of the same firm, to estimate the DAL effect. Our results confirm the evidence in Panel B of Table 3.5. Using the investment responses of the different asset classes (equipment, buildings, land) to calculate an aggregate investment response, we obtain an overall investment response of 34.9 percent, which is close to Table 3.5.

Table D.3.4: Investment types without controls

Variables	Equipment		Buildings		Land		Buildings and land share
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive	Intensive
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
DiD	**0.0375*** **	**0.0573*** **	**0.0867*** **	**0.3910*** **	**0.0215*** **	**0.4630** **	**0.0404*** **
	(0.0035)	**(0.0185)**	**(0.0052)**	**(0.0466)**	**(0.0026)**	**(0.1990)**	**(0.0027)**
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry–year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	544,085	468,458	544,085	114,285	544,085	18,175	470,228
Establishments	68,289	63,616	68,289	30,828	68,289	9,716	63,640
R-squared	0.470	0.708	0.449	0.485	0.273	0.614	0.287
Adjusted R-squared	0.393	0.662	0.370	0.295	0.169	0.168	0.176

Note: OLS regressions with establishment fixed effects and clustered standard errors at the firm level (in parentheses). For equipment (building, land) investment at the extensive margin in Model (1) (3, 5), the dependent variable is a dummy variable with a value of one for an establishment i with positive equipment (building, land) investments in t. For equipment (building, land) investment at the intensive margin in Model (2) (4, 6), the dependent variable is the logarithm of positive equipment (building, land) investments of establishment i in t. In Model (7), the dependent variable is the natural logarithm of the ratio of building plus land investments to total investments. DiD is an interaction term of a dummy variable for establishments in Eastern German states and a dummy variable for the DAL treatment period (1995–1998). ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

Table D.3.5: Lagged control variables

Investment type	Gross investment		Equipment		Buildings		Land	
Investment margin	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DiD	**0.0330*** **	**0.0880*** **	**0.0329*** **	**0.0184**	**0.0764*** **	**0.3130*** **	**0.0211*** **	**0.3850* **
	(0.0037)	**(0.0206)**	**(0.0038)**	**(0.0196)**	**(0.0057)**	**(0.0524)**	**(0.0030)**	**(0.2320)**
Establishment controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry–year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	468,549	407,241	468,549	405,481	468,549	97,531	468,549	15,522
Establishments	63,628	58,068	63,628	57,817	63,628	26,309	63,628	8,528
R-squared	0.490	0.716	0.488	0.722	0.462	0.506	0.286	0.641
Adjusted R-squared	0.412	0.668	0.409	0.675	0.380	0.309	0.176	0.183

Note: OLS regressions with establishment fixed effects and clustered standard errors at the establishment level (in parentheses). ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

Table D.3.6: Reduced sample and firm controls

Investment type	Gross investment		Equipment		Buildings		Land	
Investment margin	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DiD	**0.0250*****	**0.1000*****	**0.0266*****	**0.0339**	**0.0816*****	**0.2970*****	**0.0167*****	**0.6300***
	(0.0061)	**(0.0374)**	**(0.0044)**	**(0.0360)**	**(0.0111)**	**(0.0877)**	**(0.0064)**	**(0.3800)**
Establishment controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry–year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	192,232	175,188	192,232	174,596	192,232	54,255	192,232	8,941
Establishments	42,879	39,077	42,879	38,946	42,879	25,556	42,879	5,001
R-squared	0.568	0.798	0.568	0.802	0.551	0.561	0.372	0.642
Adjusted R-squared	0.448	0.740	0.448	0.745	0.428	0.348	0.198	0.164

Note: OLS regressions with establishment fixed effects and clustered standard errors at the establishment level (in parentheses). ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

Table D.3.7: Mixed firms with parent–year fixed effects

Investment type	Gross investment		Equipment		Buildings		Land	
Investment margin	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DiD	**0.0182**	**0.3830*** **	**0.0159**	**0.3130*** **	**0.0769*** **	**0.2990**	**0.0171** **	**0.9610**
	(0.0123)	**(0.0846)**	**(0.0124)**	**(0.0835)**	**(0.0155)**	**(0.2520)**	**(0.0084)**	**(0.8560)**
Establishment controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry–year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	37,508	29,584	37,508	29,336	37,508	6,631	37,508	492
Establishments	7,165	5,651	7,165	5,654	7,165	2,153	7,165	334
R-squared	0.490	0.716	0.488	0.722	0.462	0.506	0.286	0.641
Adjusted R-squared	0.412	0.668	0.409	0.675	0.380	0.309	0.176	0.183

Note: OLS regressions with establishment fixed effects and clustered standard errors at the establishment level (in parentheses). ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

D.5 Firm Heterogeneity and Asset Structure

In Tables D.3.8, D.3.9, and D.3.10, we report regression results using triple difference specifications as in Table 3.5, but using equipment (Table D.3.8), building (Table D.3.9), and land (Table D.3.10) investments at the extensive and intensive margins as dependent variable.

Table D.3.8: Firm heterogeneity for equipment investment

Margin	Extensive		Intensive	
Sample	Full	Reduced	Full	Reduced
	(1)	(2)	(3)	(4)
DiD	0.0260***	0.0207*	-0.0199	0.0589
	(0.00451)	(0.0122)	(0.0220)	(0.0887)
DiD Large	-0.0182*	0.0062	0.1880***	0.2470***
	(0.0103)	(0.0125)	(0.0559)	(0.0746)
DiD Group	0.0152	0.0309**	0.1030*	0.0602
	(0.0109)	(0.0152)	(0.0572)	(0.0871)
DiD Owner	0.0292*	-0.0119	0.0494	0.1410
	(0.0159)	(0.0204)	(0.0952)	(0.1290)
DiD Corp	0.0185*	-0.0015	-0.1310*	-0.1180
	(0.0108)	(0.0127)	(0.0706)	(0.0906)
Firm type controls	Yes	Yes	Yes	Yes
Establishment controls	Yes	Yes	Yes	Yes
County controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry–year FE	Yes	Yes	Yes	Yes
Observations	544,085	243,919	468,458	218,491
Establishments	68,289	45,381	63,616	42,835
R-squared	0.473	0.542	0.712	0.785
Adjusted R-squared	0.397	0.436	0.667	0.732

Note: OLS regressions with establishment, year, and industry-year fixed effects. Standard errors are clustered at the establishment level (in parentheses). Models (1) and (3) (2 and 4) refer to the extensive (intensive) margin of equipment investments, while columns (1) and (2) depict the restricted sample (only containing observations that have non-missing information on Owner-managed and Corporation), and columns (3) and (4) use the full sample. DiD is an interaction term of a dummy variable for establishments in the Eastern German states and a dummy variable for the DAL treatment period (1995–1998). DiD Large is an interaction term of a dummy variable for firms with at least 250 staff members (Large) and DiD. DiD Group is an interaction term of a dummy variable for firms with more than one establishment (Group) and DiD. DiD Owner-Managed is an interaction term of a dummy variable for firms that are managed by an owner (Owner-Managed) and DiD. DiD Corporation is an interaction term of a dummy variable for corporations with limited liability (Corporation) and DiD. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

As seen in Table D.3.8, results for equipment are widely in line with Table 3.5. We find some evidence for a stronger increase in investment activity for multi-establishment firms with the opportunity for subsidy shopping as well as strong evidence for a positive aggregate response for large firms with at least 250 employees that is primarily driven by the large increase of investments at the intensive margin for this type of firms. Hence, Table D.3.8 provides clear additional support for H3. For owner-managed firms, the evidence is relatively weak. The coefficients of DiD Owner and DiD Corporation mostly have the expected signs but are only marginally statistically significant for the full sample models.

Table D.3.9: Firm heterogeneity for building investment

Margin	Extensive		Intensive	
Sample	Full	Reduced	Full	Reduced
	(1)	(2)	(3)	(4)
DiD	0.0792***	0.0444	0.2740***	-0.0315
	(0.0066)	(0.0276)	(0.0652)	(0.222)
DiD Large	0.0035	0.0283	0.4070***	0.5740***
	(0.0157)	(0.0205)	(0.1280)	(0.1620)
DiD Group	0.0058	0.0286	-0.0698	-0.1180
	(0.0129)	(0.0203)	(0.1310)	(0.1730)
DiD Owner	0.0564**	0.0510	0.2290	0.3140
	(0.0279)	(0.0381)	(0.2280)	(0.3170)
DiD Corp	0.0087	0.0101	0.1950	0.2210
	(0.0208)	(0.0276)	(0.1690)	(0.2190)
Firm type controls	Yes	Yes	Yes	Yes
Establishment controls	Yes	Yes	Yes	Yes
County controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry–year FE	Yes	Yes	Yes	Yes
Observations	544,085	243,918	114,285	66,783
Establishments	68,289	45,381	30,828	19,964
R-squared	0.451	0.529	0.490	0.539
Adjusted R-squared	0.372	0.420	0.300	0.339

Note: OLS regressions with establishment, year, and industry-year fixed effects. Standard errors are clustered at the establishment level (in parentheses). Models (1) and (3) (2 and 4) refer to the extensive (intensive) margin of equipment investments, while columns (1) and (2) depict the restricted sample (only containing observations that have non-missing information on Owner-managed and Corporation), and columns (3) and (4) use the full sample. DiD is an interaction term of a dummy variable for establishments in the Eastern German states and a dummy variable for the DAL treatment period (1995–1998). DiD Large is an interaction term of a dummy variable for firms with at least 250 staff members (Large) and DiD. DiD Group is an interaction term of a dummy variable for firms with more than one establishment (Group) and DiD. DiD Owner-Managed is an interaction term of a dummy variable for firms that are managed by an owner (Owner-Managed) and DiD. DiD Corporation is an interaction term of a dummy variable for corporations with limited liability (Corporation) and DiD. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

For building investments, the results in Table D.3.9 are somewhat different from the results for aggregate investments in Table 3.5. Most notably, we do not find significant effects for DiD Group but likewise a very strong and positive effect for DiD Large at the intensive margin. Thus, especially investments with a very large investment response (buildings) seem to be abnormally high for large firms with low tax planning costs. This is in line with H3 and documents that large firms use bonus depreciation to a higher extent than their smaller counterparts. For land investment (Table D.3.10), there are no statistically significant triple difference interaction terms, which might be partially due to smaller observation numbers.

Table D.3.10: Firm heterogeneity for land investment

Margin	Extensive		Intensive	
Sample	Full	Reduced	Full	Reduced
	(1)	(2)	(3)	(4)
DiD	0.0199***	0.0332**	0.3060	0.1960
	(0.0033)	(0.0163)	(0.2670)	(0.8390)
DiD Large	-0.0150	-0.0149	0.9050	0.6090
	(0.0092)	(0.0133)	(0.5880)	(0.7240)
DiD Group	-0.0008	-0.0089	-0.0608	0.3780
	(0.00675)	(0.0121)	(0.5760)	(0.8070)
DiD Owner	0.0119	0.0109	0.3630	0.5210
	(0.0171)	(0.0230)	(0.7450)	(0.9740)
DiD Corp	-0.00863	-0.0149	-0.3300	-0.1110
	(0.0127)	(0.0165)	(0.6240)	(0.8530)
Firm type controls	Yes	Yes	Yes	Yes
Establishment controls	Yes	Yes	Yes	Yes
County controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry–year FE	Yes	Yes	Yes	Yes
Observations	544,085	243,919	18,175	10,737
Establishments	68,289	45,381	9,716	5,855
R-squared	0.275	0.346	0.626	0.640
Adjusted R-squared	0.170	0.196	0.170	0.166

Note: OLS regressions with establishment, year, and industry-year fixed effects. Standard errors are clustered at the establishment level (in parentheses). Models (1) and (3) (2 and 4) refer to the extensive (intensive) margin of equipment investments, while columns (1) and (2) depict the restricted sample (only containing observations that have non-missing information on Owner-managed and Corporation), and columns (3) and (4) use the full sample. DiD is an interaction term of a dummy variable for establishments in the Eastern German states and a dummy variable for the DAL treatment period (1995–1998). DiD Large is an interaction term of a dummy variable for firms with at least 250 staff members (Large) and DiD. DiD Group is an interaction term of a dummy variable for firms with more than one establishment (Group) and DiD. DiD Owner-Managed is an interaction term of a dummy variable for firms that are managed by an owner (Owner-Managed) and DiD. DiD Corporation is an interaction term of a dummy variable for corporations with limited liability (Corporation) and DiD. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

Chapter 4

Steuersatzsenkungen versus Sonderabschreibungen: Was ist die bessere Strategie zur Förderung der Standortattraktivität Deutschlands?

Steuersatzsenkungen versus Sonderabschreibungen: Was ist die bessere Strategie zur Förderung der Standortattraktivität Deutschlands?

Abstract

Wir vergleichen den Einfluss von zwei unterschiedlichen steuerpolitischen Strategien – eine dauerhafte Senkung der Unternehmenssteuerbelastung (etwa des Körperschaftsteuersatzes) und temporäre Sonderabschreibungen. Dabei greifen wir sowohl auf die empirische Literatur zum Einfluss von Unternehmenssteuern auf die Standortwahl und Investitionen als auch auf international anerkannte Indizes zur Standortattraktivität zurück. Unternehmensbefragungen, die ökonomische Literatur sowie Standortindizes legen nahe, dass der direkte Einfluss der Unternehmensbesteuerung auf die Standortattraktivität Deutschlands moderat ist. Zudem verweisen Standortindizes auf Defizite Deutschlands in standortrelevanten Bereichen (Demographie, Digitalisierung, Infrastruktur). Wir argumentieren, dass sich die steuerliche Finanzpolitik auf die Förderung von Investitionen konzentrieren sollte, um entsprechende Defizite auszugleichen (indirekte Effekte). Daher erscheinen Sonderabschreibungen als effektives und kostengünstiges Instrument der Steuerpolitik zur Förderung der Investitionstätigkeit und Standortattraktivität.

JEL classification codes: H20; H21; H23; H25

Keywords: Standortattraktivität, Investitionstätigkeit, Steuerpolitik, Wachstum

4.1. Einleitung

Bereits vor Beginn der konjunkturellen Probleme durch die Covid-19-Pandemie und den völkerrechtswidrigen Angriffskrieg Russlands in der Ukraine haben in Deutschland Forderungen nach einer strukturellen Reform der Unternehmensbesteuerung zugenommen.[1] Das Bundeswirtschaftsministerium veröffentlichte Ende 2019 vier Kernelemente einer möglichen Unternehmenssteuerreform, die neben einer Verbesserung der Thesaurierungsbegünstigung für Personengesellschaften insbesondere eine Reduktion der Unternehmenssteuerbelastung vorsahen.[2] Dies entspricht im Wesentlichen auch Forderungen des Bundesverbands der Deutschen Industrie (BDI).[3] Im Bundestagswahlkampf 2021 forderten CDU/ CSU und FDP eine Senkung der Unternehmenssteuern auf maximal 25%.[4] Kern dieser Vorschläge ist das Bestreben, die steuerliche Belastung von Unternehmen und insbesondere Kapitalgesellschaften zu senken, um im internationalen Standortwettbewerb mithalten zu können.

Vor dem Hintergrund der bestehenden Krisensituation, sowie der Stärken und Schwächen des Wirtschaftsstandortes Deutschland stellt sich die Frage, ob dies eine optimale Strategie darstellt, um dem konjunkturellen Abschwung als Folge der Wirtschaftskrisen entgegenzuwirken und zugleich die Standortattraktivität Deutschlands langfristig zu sichern. Dabei ist zu berücksichtigen, dass Steuersenkungen zu einer erheblichen Umverteilung von Steuerbelastungen führen und damit den langfristigen Trend einer steigenden Vermögens- und Einkommensungleichheit sowie die damit in Zusammenhang stehenden gesellschaftlichen Konflikte verschärfen würden.[5]

Der vorliegende Beitrag widmet sich diesem Themenkomplex aus unterschiedlichen Perspektiven. Zunächst beleuchten wir in Kapitel 4.2 Zusammenhänge zwischen der Unternehmensbesteuerung und der Standortattraktivität Deutschlands. Dabei berücksichtigen wir neben der ökonomischen Literatur zu Standortwahlentscheidungen und ausländischen Direktinvestitionen (*Foreign Direct Investment*, FDI) auch Befragungsdaten von Unternehmen und internationale Indizes zur Messung von Standortattraktivität.

Diese Indizes liefern – wie andere Ansätze – kein perfektes Maß für die Attraktivität eines Wirtschaftsstandortes. Problematisch erscheint etwa die Gewichtung der einzelnen Standortfaktoren im Gesamtindex, wobei auch Befragungen von Unternehmen

[1] Etwa Fuest und Peichl, 2020, S. 164; Homburg, 2020, S. 157f.; Hüther, 2020, S. 166f.

[2] Bundesministerium für Wirtschaft und Energie, 2019; CDU/CSU-Fraktion, 2019.

[3] Vgl. BDI, 2020.

[4] Vgl. CDU/CSU, 2021, S. 34f.; FDP, 2021, S. 6ff.; Geinitz, 2021.

[5] Vgl. für Deutschland etwa Bartels, 2018; Eichfelder, 2018; Bartels, 2019; Grabka und Halbmeier, 2019; Bach und Eichfelder, 2021, Consiglio et al., 2021, Stockhausen und Maiworm, 2021, Grabka, 2022, sowie die umfassendere internationale Diskussion um Steuern und Verteilungsfragen bei Atkinson, 2014; Piketty, 2014; Saez und Zucman, 2019; Case und Deaton, 2020; Banerjee und Duflo, 2021.

für ein relativ geringes Gewicht des Faktors Unternehmensbesteuerung sprechen.[6] Ein wichtiges Argument für die Berücksichtigung von internationalen Standortindizes liegt darin, dass die vorliegende Literatur zum Einfluss der Unternehmensbesteuerung kaum Rückschlüsse über die Relevanz von Steuern im Verhältnis zu anderen Standortfaktoren zulässt, sondern sich praktisch ausschließlich auf steuerliche Aspekte fokussiert. Damit lässt sich letztlich nur eine Debatte über die Position Deutschlands in einem rein steuerlichen Standortwettbewerb führen. Dies greift allerdings zu kurz, da andere (wichtigere) Standortfaktoren vernachlässigt werden. Im Grunde entspricht dieses Argument der Einsicht, dass Steuerplanung im weiteren Sinne den Kapitalwert nach Steuern maximieren und nicht etwa den Barwert der Steuerzahlungen minimieren sollte. Dementsprechend sollte steuerliche Standortpolitik die Wettbewerbsposition Deutschlands im Standortwettbewerb und nicht nur dessen Wettbewerbsposition in einem rein auf steuerliche Aspekte verengten Steuerwettbewerb verbessern.[7]

Standortindizes sind im wirtschaftspolitischen Diskurs relevante Größen (etwa der *Global Competitiveness Report* des *World Economic Forum*, WEF, in Davos), die die Wahrnehmung des Wirtschaftsstandortes Deutschland beeinflussen. Eine interessante Beobachtung ist, dass international übliche Indizes wesentlich mehr Einzelfaktoren berücksichtigen als im Rahmen von steuerökonomischen Studien als Kontrollvariablen verwendet werden. So berücksichtigt etwa das *World Competitiveness Yearbook* des *International Institute für Management Development* IMD 337 Einzelfaktoren, während Barrios et al. (2012) in einer Querschnittsuntersuchung 5 steuerliche Variablen und 5 nichtsteuerliche Kontrollvariablen verwenden.[8] Diese Beobachtung verweist auf das Risiko, dass ein rein steuerlich motivierter Diskurs zu Fragen der Standortattraktivität die Relevanz anderer Faktoren vernachlässigt oder unterschätzt.[9] International übliche Standortindizes machen deutlich, dass Deutschland eine starke Position im internationalen Standortwettbewerb innehat. Nach dem aktuellen *Global Competitiveness Index* (GCI) des Weltwirtschaftsfo-

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[6]Vgl. MacCarthy und Atthirawong, 2003; Ebertz et al., 2008, S. 18; Ernst Young, 2011, S. 9; Kimelberg und Williams, 2013; Vlachou und Iakovidou, 2015; Landua et al., 2017; Ernst Young, 2021, S. 43.

[7]Im Hinblick auf die Frage der Standortattraktivität erscheint der Begriff „Steuerwettbewerb“ dabei nicht ganz unproblematisch, da er suggeriert, dass Steuern einen besonders wichtigen Standortfaktor darstellen.

[8]Vgl. Barrios et al., 2012, S. 956f.; IMD, 2020b, S. 3. Auch andere Beiträge zum Einfluss der Besteuerung auf die Standortwahl verwenden eine überschaubare Anzahl von Kontrollvariablen; vgl. etwa Hebous, Ruf und Weichenrieder, 2011, S. 827 (8 Kontrollvariablen); Crabbé und De Bruyne, 2013, S. 435 (8 Kontrollvariablen); Lawless et al., 2018, S. 2926 (8 Kontrollvariablen). Dabei können Studien zur Standortwahl regelmäßig auch nicht auf unternehmens- oder länderspezifische fixe Effekte kontrollieren, was das Problem weiter verschärft.

[9]Dies gilt insbesondere unter Berücksichtigung des Hauptergebnisses der Literatur zu Agglomerationseffekten, nach der nichtsteuerliche Agglomerationsvorteile eines Standortes (etwa Netzwerkeffekte oder Infrastruktur) die Relevanz von Steuern auf die Standortwahl abmildern; vgl. Devereux, Griffith und Simpson, 2007; Brülhart, Jametti und Schmidheiny, 2012; Crabbé und De Bruyne, 2013; Luthi und Schmidheiny, 2014; Brülhart und Simpson, 2018 mit weiteren Nachweisen.

rums liegt Deutschland global auf Rang 7 (im Vorjahr Rang 3) und in Europa auf Rang 3 (im Vorjahr Rang 1).[10] Die Analyse macht aber auch deutlich, dass Deutschland Defizite in einzelnen Bereichen aufweist. Nach dem GCI umfassen diese insbesondere Adoption von Informations- und Kommunikationstechnologien (inkl. Mobilfunk- und Glasfasernetze), Finanzsystem (Bankengesundheit, Kredit-Gap), Gesundheit, Steuerlast des Faktors Arbeit, Lohn- und Arbeitsflexibilität, Komplexität von Zöllen, Regulierung von Interessenkonflikten und die organisierte Kriminalität.[11] Die Analyse der Standortindizes macht deutlich, dass zusätzliche Investitionen (etwa in Mobilfunk- und Glasfasernetze) eher als Steuersatzsenkungen dazu geeignet sind, Defizite Deutschlands auszugleichen und dessen Standortattraktivität zu verbessern.

In Kapitel 4.3 widmen wir uns daher Zusammenhängen zwischen der Unternehmensbesteuerung und der Investitionstätigkeit von Unternehmen. Auf Basis der empirischen Literatur beschäftigen wir uns insbesondere mit der Stärke des steuerlichen Einflusses. Dabei vergleichen wir den in der Literatur geschätzten Effekt von allgemeinen Steuersatzsenkungen, die üblicherweise im Rahmen der Kapitalnutzungskosten von realen Investitionen (*user cost of capital*) gemessen werden, mit dem Effekt von gezielten steuerlichen Investitionsanreizen in Form von Sonderabschreibungen.

In Kapitel 4.4 diskutieren wir abschließend die Effektivität von zwei unterschiedlichen steuerpolitischen Strategien zur Erhöhung der Standortattraktivität: a) der Senkung der prozentualen Unternehmenssteuerbelastung und b) der gezielten Steigerung von Investitionen in Bereichen, in denen die Bundesrepublik Defizite aufweist, oder in denen ein dringender Investitionsbedarf absehbar ist (Energienetze, Bildungssystem, etc.). Dieser Investitionsbedarf wird insbesondere durch die sogenannten „Mega-Trends“ Digitalisierung, demographischer Wandel und Dekarbonisierung getrieben. Die bisher vorhandene Evidenz deutet darauf hin, dass eine Strategie der gezielten Investitionsförderung besser zur Steigerung der Standortattraktivität geeignet ist. Da der direkte Einfluss der Unternehmensbesteuerung auf die anhand von Indizes gemessene Standortattraktivität moderat ist, kommt es vor allem auf die indirekten Effekte an, die durch den Einfluss der Steuern auf die Investitionstätigkeit ausgelöst werden. Hier verdeutlichen empirische Studien, dass zeitlich begrenzte Sonderabschreibungen einen starken Einfluss auf Investitionen entfalten. Somit erscheint es durchaus wünschenswert, dass der Koalitionsvertrag der Ampelkoalition sogenannte „Superabschreibungen“ in den Bereichen Klimaschutz und Digitalisierung vorsieht, auch wenn der offenbar aus Marketinggesichtspunkten verwendete Begriff vielleicht etwas irritieren mag.[12]

Unsere Argumentation ist nicht einzigartig im ökonomischen Schrifttum. In Diskus-

[10] Vgl. WEF, 2018, S. 239f.; WEF, 2019, S. 238ff.

[11] Vgl. WEF, 2019, S. 238ff.

[12] Vgl. SPD, BÜNDNIS 90/DIE GRÜNEN, FDP (2021), S. 164.

sionen über potentielle Maßnahmen zur konjunkturellen Stabilisierung in Deutschland wurden zeitlich befristete Sonderabschreibungen mehrfach vorgeschlagen.[13] In den Kieler Konjunkturberichten des Instituts für Weltwirtschaft wird geschlussfolgert, dass zielgenaue, selbstdosierende und reaktionsschnelle Maßnahmen (wie Kreditbürgschaften und die Stundung von Abgaben) besser geeignet sind als eine Senkung der Unternehmenssteuern, um die Liquidität der Unternehmen zu sichern und die Konjunktur zu stabilisieren.[14] Darüber hinaus betont eine Simulationsstudie von Koch und Langenmayr (2020) die Notwendigkeit von Investitionsanreizen für eine gesamtwirtschaftliche Erholung im Rahmen der Covid-19-Pandemie und empfiehlt eine Ausweitung des Verlustrücktrags aufgrund der asymmetrischen steuerlichen Behandlung von Gewinnen und Verlusten.[15] Eine Simulationsstudie des Ifo-Instituts kommt ebenso wie wir zu dem Ergebnis, dass beschleunigte Abschreibungen mit Abstand am besten geeignet sind, um die Investitionstätigkeit, das Bruttoinlandsprodukt und die Beschäftigung zu erhöhen.[16]

Unser Beitrag erweitert die Literatur dennoch in mehrfacher Hinsicht. Zunächst bieten wir einen umfassenden Überblick zu Befragungsdaten sowie zur ökonomischen Literatur zu den Themen Standortwahl und Agglomerationseffekten und beleuchten deren Ergebnisse und methodischen Herausforderungen kritisch. Ein besonderer Beitrag unseres Papiers liegt darin, dass wir explizit Bezug auf allgemein anerkannte Indizes zur Messung der Standortattraktivität nehmen und damit auf Maßgrößen abstellen, die die Stärken und Schwächen des Wirtschaftsstandortes Deutschland herausarbeiten. Wir können damit die steuerliche Wettbewerbsposition Deutschlands in ein Verhältnis zu anderen relevanten Bereichen setzen und somit eine umfassendere Perspektive einnehmen als dies Studien möglich ist, die sich ausschließlich auf steuerliche Faktoren konzentrieren. Uns ist kein weiterer Beitrag bekannt, der ein entsprechendes Vorgehen aufweist. Eine Verknüpfung der daraus resultierenden Ergebnisse mit der empirischen Literatur zum Einfluss von Unternehmenssteuern auf Standortwahl und Investitionen führt uns zu dem Punkt, dass vor allem Sonderabschreibungen geeignet sind, um Investitionen in Bereichen, in denen Deutschland Defizite aufweist, anzuregen. Zudem könnten auch öffentliche Direktinvestitionen ein geeignetes Mittel darstellen, um die Wettbewerbsposition Deutschlands in Bereichen wie Digitalisierung, Demographie und Dekarbonisierung zu verbessern.

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[13]Vgl. etwa Dullien et al., 2019, S. 748ff.

[14]Vgl. Boysen-Hogrefe et al., 2020, S. 27ff.

[15]Vgl. Koch und Langenmayr, 2020, S. 367.

[16]Vgl. Dorn et al., 2021, S. 8. Die Studie verwendet ein computable general equlilibrium (CGE) model und zeichnet sich durch ein rein theoretisches Vorgehen aus. Im Unterschied zum vorliegenden Beitrag geht das Papier also nicht explizit auf empirische Untersuchungen ein.

4.2. Direkte Effekte der Besteuerung auf die Standortattraktivität

4.2.1. Auswertung der Literatur

4.2.1.1. Unternehmenssteuern und Standortwahl

Die theoretische Basis für die steuerliche Literatur zu Standortwahlentscheidungen bieten Devereux und Griffith (2003) mit einem Partialmodell, in dem die *Effective Average Tax Rate* (EATR) als Maßgröße für Standortwahlentscheidungen hergeleitet wird. Im Rahmen dieses Modells lässt sich zeigen, dass die Relevanz von Steuersätzen insbesondere mit der Rentabilität von Investitionsprojekten steigt, während „marginale“ Investitionen (Kapitalwert nahe null) stärker durch Abschreibungsregeln beeinflusst werden.[17] Ein Nachteil des Modellansatzes liegt darin, dass nichtsteuerliche Standortfaktoren und deren Interaktion mit steuerlichen Parametern nicht explizit modelliert oder untersucht werden.

Die ökonometrische Literatur liefert zahlreiche Belege für die Theorie von Devereux und Griffith, dass Unternehmenssteuern Standortwahlentscheidungen beeinflussen.[18] Bezüglich der Relevanz von Unternehmenssteuern gibt es aber durchaus heterogene Ergebnisse. Devereux und Griffith (1998) können in ihrer grundlegenden Studie zum Einfluss von effektiven Steuerbelastungen auf Standortwahlentscheidungen US-amerikanischer Unternehmen zwar einen signifikanten Zusammenhang zwischen der Steuerlast und dem Ort einer Niederlassung innerhalb Europas feststellen. Für die Frage, ob ein US-Unternehmen nach Europa expandiert, können Sie aber keinen signifikanten Steuereffekt identifizieren.[19] Gius und Frese (2002) finden keinen signifikanten Zusammenhang zwischen Unternehmenssteuern und der Wahl eines Unternehmensstandortes in den USA[20] und Devereux, Griffith und Simpson (2007) nur einen schwachen Einfluss von lokalen Zuschüssen.[21] Einen Beleg für wenig robuste Ergebnisse dieser Literatur liefert die Studie von Becker, Egger und Merlo (2012), die den Einfluss der Gewerbesteuerhebesätze auf die Standortwahl internationaler Unternehmen in Deutschland untersucht. Die Autoren finden ausgesprochen hohe steuerliche Effekte in Spezifikationen, in denen ein problematischer und methodisch angreifbarer Instrumentalvariablenansatz (IV) verwendet wird. Werden hingegen übliche OLS-Schätzverfahren (Ordinary Least Squares bzw. kleinste Quadrate-Schätzer) gewählt,

[17] In Übersteinstimmung mit diesem Ergebnis finden Eichfelder, Jacob und Schneider (2020), S. 6, 24ff. Belege, dass Sonderabschreibungen sich stärker auf die Qualität von Investitionen von wenig produktiven Unternehmen auswirken als bei hoch produktiven Unternehmen.

[18] Vgl. etwa Devereux und Griffith, 1998; Coughlin und Segev, 2000; Hebous, Ruf und Weichenrieder, 2011; Crabbé und De Bruyne, 2013; Barrios et al., 2012; Lawless et al., 2018; Xiao und Wu, 2020.

[19] Vgl. Devereux und Griffith, 1998, S. 362f.

[20] Vgl. Gius und Frese, 2002, S. 47-48.

[21] Vgl. Devereux, Griffith und Simpson, 2007, S. 413f.

betragen die steuerlichen Effekte nur etwa ein Zehntel der IV-Schätzung.[22]

Eine theoretische Begründung für moderate und heterogene Effekte der Unternehmensbesteuerung auf die Standortwahl bietet die Neue Ökonomische Geographie, für die Paul Krugman mit dem Alfred-Nobel Gedächtnispreis der Schwedischen Reichsbank für Wirt- schaftswissenschaften ausgezeichnet wurde. Nach dieser Theorie können Steuern als Preis für die Attraktivität einer wirtschaftlichen Region interpretiert werden.[23] Räume und Volkswirtschaften mit einer hohen Standortattraktivität können höhere Steuern am Markt für Investitionen durchsetzen, während sich wirtschaftlich weniger attraktive Regionen nur geringfügige Steuerbelastungen leisten können. Eine eindrucksvolle anekdotische Evidenz für diese Effekte bietet die Verteilung von Risikokapitalinvestitionen in den USA. Obwohl für Start-ups attraktive Bundesstaaten wie New York, Massachusetts und insbesondere Kalifornien relativ hohe Belastungen durch *Corporate State Income Taxes*[24] zwischen 7,1% und 8,84% aufweisen, entfielen auf diese drei Staaten 73,0% aller Risikokapitalinvestitionen in den USA im Jahr 2020 (alleine 51,3% auf Kalifornien). Demgegenüber spielen ökonomisch erfolgreiche Staaten wie Texas oder Florida mit Anteilen von 2,9% sowie 1,1% trotz nicht erhobener bzw. geringer *Corporate State Income Tax* in diesem Markt nur eine untergeordnete Rolle.[25] Dies dokumentiert den starken Einfluss von Agglomerationseffekten (insbesondere im Silicon Valley) auf den Markt für Risikokapitalinvestitionen in den USA.

Eine Reihe empirischer Beiträge findet Belege für diese theoretischen Überlegungen.[26] Brülhart, Jametti und Schmidheiny (2012) nutzen lokale Steuersatzvariationen zwischen Schweizer Kantonen als Steueranreiz und Unterschiede in Branchenkonzentrationen (EG-Index) als Maß für Agglomerationseffekte. Sie können zeigen, dass die Standortwahl in Industriezweigen mit starker Konzentration in einzelnen Regionen weniger

[22]Geeignete Instrumentalvariablen zeichnen sich dadurch aus, dass diese keinen direkten Einfluss auf die abhängige Variable (Standortwahl), sondern nur einen direkten Einfluss auf die endogene erklärende Variable (Steuersatz) aufweisen. Becker, Egger und Merlo, 2012, S. 707ff. verwenden als Instrumentalvariable den durchschnittlichen Hebesatz (= Komponente des Steuersatzes) der umliegenden Gemeinden und argumentieren, dass dieser über den Steuerwettbewerb den Hebesatz der Gemeinde i beeinflusst. Leider vernachlässigen sie in ihrer Argumentation, dass der Hebesatz der Nachbargemeinden von i auch direkt die Standortwahlentscheidung beeinflusst, da das Unternehmen x sich anstelle der Gemeinde i auch in einer Nachbargemeinde von i niederlassen könnte. Der Hebesatz als Maß für den Steuersatz der Nachbargemeinden ist folglich ein direkter Einflussfaktor in der Standortentscheidung, womit der IV-Ansatz des Papiers nicht zu einer konsistenten Schätzung kausaler Steuereffekte auf die Standortwahl führen dürfte.

[23]Vgl. Krugman, 1991; Baldwin und Krugman, 2004; Hühnerbein und Seidel, 2010.

[24]Die *Corporate State Income Tax* wird von US-amerikanischen Bundesstaaten zusätzlich zur Bundessteuer (*Federal Corporate Income Tax*) erhoben. Die *Corporate State Income Taxes* betragen 7,1% für New York, 8,84% für Kalifornien und 8% für Massachusetts sowie jeweils 0% für Florida und Texas; vgl. `http://www.tax-rates.org/taxtables/corporate-income-tax-by-state` (01.09.2021).

[25]Vgl. `https://www.statista.com/statistics/424167/venture-capital-investments-usa-by-state/` (01.09.2021).

[26]Vgl. etwa Devereux, Griffith und Simpson, 2007; Brülhart, Jametti, und Schmidheiny, 2012; Crabbé und De Bruyne, 2013; Luthi und Schmidheiny, 2014; Fréret und Maguain, 2017; Brülhart und Simpson, 2018; Xiao und Wu, 2020 mit weiteren Nachweisen.

stark bis überhaupt nicht (im Falle der Schweizer Uhrenindustrie) mit Steuersätzen korreliert ist, während bei weniger konzentrierten Branchen (Softwareindustrie) deutlich stärkere Korrelationen bestehen. Auch in dieser Studie zeigt sich eine hohe Heterogenität der Schätzergebnisse (insbesondere bei Schätzungen mit Instrumentalvariablen). Je nach Modell stellen die Autoren fest, dass mit Hilfe des EG-Index gemessene Agglomerationseffekte den Steuereffekt halbieren oder gar auf effektiv null reduzieren.[27] Dies interpretiert die Literatur als Belege für sogenannte Agglomerationsrenten (also nicht reproduzierbare Agglomerationsvorteile), die sich ökonomisch effizient durch den Staat besteuern lassen.

Im Folgenden soll auf einige methodische Probleme der ökonomischen Literatur zum Einfluss der Unternehmensbesteuerung auf die Standortwahl verwiesen werden. Ein grundlegendes Problem besteht darin, dass Unternehmen ihre Standorte nur sehr selten verlagern. Dementsprechend konzentrieren sich die vorhandenen Studien in aller Regel auf den Zusammenhang von Unternehmensneugründungen und dem Steuersatz. Allerdings können derartige Modelle regelmäßig nur die Korrelation zwischen dem Steuersatz und der Wahrscheinlichkeit, einen bestimmten Standort zu wählen, aber nicht die Wirkungsrichtung feststellen. Dementsprechend ist es wenig erstaunlich, dass Ergebnisse teils stark von der Parameterspezifikation und der Auswahl der Kontrollvariablen abhängen. Barrios et al. (2012) finden etwa keine signifikanten Steuereffekte, wenn keine Kontrollvariablen verwendet werden und zeigen auch keine Robustheitstests für alternative Spezifikationen von Kontrollvariablen. Die Ergebnisse können mithin stark durch das Variablensetting getrieben sein.[28] Lawless et al. (2018) finden nur dann signifikante steuerliche Effekte, wenn quadratische Steuerterme berücksichtigt werden.[29] Letztlich lässt sich festhalten, dass reine Querschnittsmodelle regelmäßig keine kausale Interpretation zulassen und entsprechende Ergebnisse vorsichtig verwendet werden sollten.

Einen erfolgsversprechenden Lösungsansatz verwenden Giroud und Rauh (2019), die Daten über die Zahl der Betriebe auf Ebene der Firmen und der US-Bundesstaaten aggregieren. Dieser Ansatz erlaubt die Verwendung von fixen Effekten für Kombinationen aus Bundesstaat und Firma (*state-year fixed effects*) und ist somit robuster im Hinblick auf Endogenität als herkömmliche Schätzansätze. Mit Hilfe dieses Ansatzes kommen Giroud und Rauh (2019) zu dem Ergebnis, dass eine Erhöhung des Steuersatzes um einen Prozentpunkt (im Falle der *US State Income Tax* eine durchaus hohe Variation des Steuersatzes) zu einer Reduktion der Zahl der Betriebe um 0,4 bis 0,5 Prozent bei Kapitalgesellschaften und um 0,2 bis 0,3 Prozent bei Personengesellschaften führt.[30]

Ein weiteres Problem besteht darin, dass die Literatur regelmäßig nicht zwischen der

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[27]Vgl. Brülhart, Jametti, und Schmidheiny, 2012, S. 1077ff., 1082ff.

[28]Vgl. Barrios et al., 2012, S. 956f.

[29]Vgl. Lawless et al., 2018, S. 2925.

[30]Vgl. Giroud und Rauh, 2019, S. 1281f., 1286f.

Standortwahl zu Zwecken der Steuerplanung und der Standortwahl für realwirtschaftliche Zwecke differenziert.[31] Bereits Slemrod (1995) verweist darauf, dass Unternehmen auf steuerliche Anreize gerade auch mit aggressiver Steuerplanung (Gewinnverlagerung, Steuerbilanzpolitik) reagieren, da dies zumeist kostengünstiger ist als eine Anpassung des Geschäftsmodells.[32] Aus der umfassenden Literatur zu internationalen Gewinnverlagerungen ist bekannt, dass Unternehmen Tochtergesellschaften in Niedrigsteuerländern nutzen, um Gewinne aus Hochsteuerländern zu verlagern.[33] Dies wird durch anekdotische Beispiele unterstrichen. Die DB Industrial Holdings GmbH, eine Tochtergesellschaft der Deutschen Bank mit Sitz im Feuerwehrhaus der deutschen Gewerbesteueroase Lützen (Hebesatz 240%), weist in ihrem Jahresabschluss 2020 Sachanlagen und Beteiligungen von 40,20 € (davon Betriebs- und Geschäftsausstattung 28,00 €) und Forderungen gegen die Deutsche Bank AG in Frankfurt am Main (Hebesatz 460%) von 1,575 Mrd. € auf.[34] Es wird deutlich, dass der Zweck dieser „Briefkastenfirma" offenbar in der Gewinnverlagerung in eine Steueroase besteht. Dennoch dürften zahlreiche bisherige empirische Studien diese und ähnliche Gestaltungen als eine „reale" Standortentscheidung interpretieren.

Beiträge aus der Accounting-Literatur verweisen zudem darauf, dass aus steuerlichen Zwecken gezielt Holdingstrukturen optimiert werden.[35] So zeigt etwa Rünger (2019), dass die Einführung der österreichischen Gruppenbesteuerung zu einer Erhöhung der Anzahl von Zwischengesellschaften in Österreich geführt hat. Internationale Unternehmen in Österreich haben also bewusst zusätzliche Tochtergesellschaften in Österreich gegründet, um die Vorteile der grenzüberschreitenden Gruppenbesteuerung besser für sich nutzen zu können. Sowohl die bisherige Literatur als auch anekdotische Evidenz zu „Briefkastenfirmen" machen deutlich, dass Unternehmen Tochtergesellschaften in Niedrigsteuergebieten gründen, um Steuern zu sparen, ohne dabei reale Geschäftsmodelle anzupassen. Die Nichtberücksichtigung derartiger Gestaltungen in der steuerlichen Literatur zur Standortwahl dürfte dazu führen, dass Steuereffekte auf reale Standortentscheidungen tendenziell überschätzt werden.

Ein weiterer Schwachpunkt besteht darin, dass sich die Diskussion bislang fast ausschließlich auf den direkten Effekt der Unternehmenssteuern auf die Standortwahl[36] sowie auf dessen Moderation durch Agglomerationseffekte[37] beschränkt, wobei Korrelationen

[31]Dies gilt auch für Beiträge, die Bezüge zur Literatur steuerlicher Gewinnverlagerungen nehmen; vgl. etwa Barrios et al., 2012 oder Merlo, Riedel und Wamser, 2020.

[32]Vgl. Slemrod, 1995, S. 176ff.

[33]Einen Literaturüberblick bietet Dharmapala, 2014.

[34]Vgl. DB Industrial Holdings GmbH, Jahresabschluss zum Geschäftsjahr vom 01.01.2020 bis zum 31.12.2020, `https://www.bundesanzeiger.de/pub/de/start?5` sowie Riemer (2022).

[35]Vgl. etwa Lewellen und Robinson, 2013; Dyreng et al., 2015; Rünger, 2019.

[36]Vgl. etwa Devereux und Griffith, 1998; Gius und Frese, 2002; Hebous, Ruf und Weichenrieder, 2011; Barrios et al., 2012; Lawless et al., 2018; Merlo, Riedel und Wamser, 2020 mit weiteren Nachweisen.

[37]Vgl. etwa Devereux, Griffith und Simpson, 2007; Brülhart, Jametti, und Schmidheiny, 2012; Crabbé

häufig auch kausal interpretiert werden. Dies dürfte auch dadurch bedingt sein, dass sich theoretische Modelle im Sinne von Devereux und Griffith (2003) praktisch ausschließlich auf den direkten Steuereinfluss konzentrieren. Damit bleiben aber wesentliche Fragen unberücksichtigt. So ist bislang unklar, in welchem Verhältnis Unternehmenssteuern zu anderen Standortfaktoren stehen und inwieweit Steuern andere Standortfaktoren beeinflussen und deren Effekte moderieren können. Dieser Befund ist insofern überraschend, als Befragungsdaten (siehe Abschnitt 4.2.1.3) und Standortindizes (siehe Abschnitt 4.2.2) eine hohe Bedeutung von nichtsteuerlichen Standortfaktoren suggerieren. Da die steuerlich motivierte ökonomische Literatur diesbezüglich aber nur wenige Erkenntnisse bietet und nur vereinzelt für wesentliche Faktoren wie Arbeitskosten (z.B. das Lohnniveau), Qualität der Arbeitskräfte (Bildungsgrad und –struktur), Infrastruktur (z.B. Transportkosten, Infrastrukturausgaben und Indexscores) und sogar politische Stabilität (z.B. über Korruptionsindizes) oder Lebensqualität kontrolliert, gehen wir in Abschnitt 4.2.2 auf international übliche Indizes als Maßgrößen für Standortattraktivität ein.

4.2.1.2. Literatur zu Unternehmenssteuern und ausländischen Direktinvestitionen

Ein weiterer Literaturzweig, der sich mit dem Einfluss der Besteuerung auf die Attraktivität von Standorten befasst, ist die Literatur zu ausländischen Direktinvestitionen (*Foreign Direct Investments*, FDI). Dabei wird unterstellt, dass Steuern internationale Unternehmen dahingehend beeinflussen, an welchem Standort Direktinvestitionen getätigt werden. Im Rahmen von Metastudien ermittelte durchschnittliche Semi-Elastizitäten für ausländische Direktinvestitionen liegen zwischen -2,5 und -3,3.[38] Eine Semi-Elastizität bezeichnet die prozentuale Veränderung der Investitionstätigkeit, die sich bei der Veränderung der effektiven Steuerlast um einen Prozentpunkt ergibt. Diese durchaus hohen Durchschnittseffekte verdecken allerdings eine hohe Varianz der Ergebnisse einzelner Studien, was etwa anhand einer Metastudie von Feld und Heckemeyer (2011) deutlich wird. Eine hohe Bedeutung hat in diesem Zusammenhang die verwendete Maßgröße für FDI.

Die durchschnittliche Elastizität von -2,55 sowie die Median-Elastizität von -2,49 werden bei Feld und Heckemeyer (2011) erheblich durch Studien beeinflusst, die FDI anhand von einfachen Stromgrößen mit Hilfe von Länderdaten (FDI Stock, FDI-Zuflüsse) messen. Diese FDI-Kennzahlen werden allerdings nicht nur durch Realinvestitionen, sondern auch durch aggressive Steuergestaltungen getrieben. So weist etwa das als Steueroase bekannte Land Luxemburg mit 62 Mrd. US $ im Jahr 2020 einen der höchsten FDI-Zuflüsse der gesamten OECD-Staaten auf, der die Summe aus dem FDI-Zufluss von

und De Bruyne, 2013; Fréret und Maguain, 2017; Brülhart und Simpson, 2018; Xiao und Wu, 2020 mit weiteren Nachweisen.

[38] Vgl. die Metaanalysen von De Mooij und Ederveen, 2003, S. 673f.; De Mooij und Ederveen, 2008, S. 695; Feld und Heckemeyer, 2011, S. 233f.

Frankreich (22,0 Mrd. US $) und Großbritannien (19,7 Mrd. US $) übersteigt.[39] Es dürfte offenkundig sein, dass dieser Zufluss an Kapital nicht durch Realinvestitionen getrieben ist.[40] Dementsprechend überschätzen Studien, die allein unbereinigte Länderdaten analysieren, den Einfluss von Steuern auf reale ausländische Direktinvestitionen. Auch wenn Mikrodaten ebenfalls durch Gewinnverlagerungen in Niedrigsteuerländer beeinflusst sein können (so führen etwa langfristige Forderungen gegenüber verbundenen Unternehmen zu mehr Anlagevermögen einer Tochtergesellschaft in einer Steueroase), dürfte der Schätzfehler mit diesen Daten dennoch geringer ausfallen als mit aggregierten Daten. Dies verdeutlicht auch die Metaanalyse von Feld und Heckemeyer (2011). Die Autoren ermitteln für Studien, die FDI nicht mit aggregierten Daten sondern mit Hilfe von Mikrodaten auf Firmenebene messen, einen negativen und signifikanten Koeffizienten im Betrag von 1,978. Dies bedeutet, dass ceteris paribus die geschätzte Semi-Elastizität von -2,55 auf nur noch -0,57 sinkt, wenn mit Mikrodaten anstelle von aggregierten Daten gearbeitet wird. Dementsprechend kommt es zu einer Überschätzung des Steuereffekts, wenn ohne Korrektur mit Makrodaten (FDI *Flows*) gearbeitet wird.

Ein weiteres gravierendes Problem in empirischen Analysen zum Einfluss der Unternehmensbesteuerung auf aggregierte FDI mit Hilfe von Länderdaten ist – ähnlich auch zu Studien zur Standortwahl – Endogenität. Die Schätzgleichungen entsprechender Studien haben in der Regel folgende Struktur

$$FDI_{ct} = \alpha_0 + \alpha_1 \cdot \tau_{ct} + \beta \cdot X_{ct} + \epsilon_{ct} \tag{4.1}$$

wobei FDI_{ct} die Kapitalzuflüsse im Land c im Jahr t, τ_{ct} den Steuersatz des entsprechenden Landes, X_{ct} einen Vektor mit Kontrollvariablen des Landes und ϵ_{ct} einen Störterm bezeichnen. Als Kontrollvariablen werden üblicherweise Daten wie BIP, BIP pro Einwohner, Einwohnerzahl, Wachstum des BIP und Ähnliches verwendet. Es liegt auf der Hand, dass die Kausalität der Schätzung hier nicht klar ist, da ausländische Direktinvestitionen das BIP (und dessen Wachstum) beeinflussen und umgekehrt. Das Wachstum des BIP kann dann wiederum den Steuersatz beeinflussen. Generell sollten daher Ergebnisse von Schätzungen mit Hilfe von aggregiertem BIP auf Länderebene ausgesprochen vorsichtig interpretiert werden.

Es gibt noch weitere Gründe, die für eine Überschätzung der Elastizität von realen Auslandsinvestitionen durch die vorliegenden Studien sprechen: 1) Ausländische Direktin-

[39] Vgl. OECD (2020).

[40] Aggressive Steuergestaltungen im Rahmen von Gewinnverlagerungen führen dazu, dass hohe Volumina an Cash und an immateriellen Vermögensgegenständen in Steueroasen wie etwa Luxemburg gehalten werden (vgl. auch Dharmapala, 2014; Zucman, 2015). Dies führt zwar zu einer Zunahme der FDI in diesen Ländern sowie zu einer Senkung der Steuerschuld für das Unternehmen, hat aber praktisch kaum Auswirkungen auf reale Investitionen und Geschäftsmodelle. Es handelt sich somit schlicht um aggressive Steuergestaltungen, die sich auf das in OECD-Statistiken ausgewiesene FDI auswirken.

vestitionen sind nur ein Teilbereich der gesamten Investitionstätigkeit und dürften elastischer auf Steuerlastdifferenzen reagieren als heimische Investitionen. 2) Feld und Heckemeyer (2011) finden Belege dafür, dass der Schätzwert aufgrund von Selektionseffekten überschätzt wird (sogenannter *Publication Bias*), korrigieren den Durchschnitt und den Median aber nicht auf diesen Schätzfehler.[41] 3) Selbst wenn Investitionen mit Hilfe von Bilanzdaten von Unternehmen gemessen werden, können Steuereffekte durch Steuergestaltungen und nicht durch reale Reaktionen getrieben sein. So findet die Accounting-Forschung starke Belege für steuerlich getriebene Bilanzpolitik.[42] Wir greifen diese Punkte noch einmal auf, wenn wir indirekte Effekte auf die Standortattraktivität über den Kanal der Investitionen diskutieren (Kapitel 4.3).

4.2.1.3. Unternehmensbefragungen

Befragungen von Unternehmen kommen regelmäßig zu dem Ergebnis, dass Steuern zwar einen wesentlichen, aber nicht den wichtigsten Standortfaktor darstellen. Nach einer Metastudie des Ifo-Instituts Dresden von Ebertz et al. (2008) werden Steuern und Abgaben als fünftwichtigster Faktor für die Standortwahl zwischen deutschen Kommunen eingeordnet, der bei Unternehmensneugründungen aber auch bei Umstrukturierungen und Standortverlagerungen relevant ist.[43] Größere Bedeutung haben das Angebot an qualifizierten Arbeitskräften, die Kundennähe, das Lohnniveau und die Verkehrsanbindung bzw. Infrastruktur. Eine Studie der Kreditanstalt für Wiederaufbau (2017), die deutlich mehr harte und weiche Standortfaktoren berücksichtigt, sieht die Besteuerung sogar eher im unteren Mittelfeld der relevanten Faktoren.[44] In einer aktuellen Studie von Ernst Young (2021) werden in Zeiten der Corona-Krise makroökonomische Stimuli (zu denen auch steuerliche Anreize gehören) als neunter von neun wichtigen Standortfaktoren genannt. Wichtiger waren etwa die Stabilität des politischen und regulatorischen Regimes, Arbeitskräfte und Infrastruktur, Kosten, Stärke des Heimatmarktes, Lebensqualität und Kultur sowie der Politikansatz im Hinblick auf Klimawandel und Nachhaltigkeit.[45] Weitere internationale Unternehmensbefragungen kommen hinsichtlich der Relevanz von Steuern zu ähnlichen Ergebnissen.[46]

[41] Der *Publication Bias* bezeichnet den Effekt, dass Wissenschaftler dazu neigen, ausschließlich signifikante bzw. mit der Theorie übereinstimmende Forschungsergebnisse zu veröffentlichen, was im Ergebnis zu überhöhten Schätzungen in veröffentlichten Studien führt; vgl. Feld und Heckemeyer, 2011, S. 233f.

[42] Hohe Steuersätze führen insbesondere dazu, dass Unternehmen einen stärkeren Anreiz haben, überhöhte steuerwirksame Abschreibungen zu tätigen oder aktivierte Vermögensgegenstände niedriger zu bewerten; vgl. hierzu etwa Scholes, Wilson und Wolfson, 1992; Sundvik, 2017; Badertscher et al., 2019; Eichfelder et al., 2020.

[43] Vgl. Ebertz et al., 2008, S. 18.

[44] Vgl. Landua et al., 2017.

[45] Vgl. Ernst Young, 2021, S. 43.

[46] Vgl. etwa MacCarthy und Atthirawong, 2003; Kimelberg und Williams, 2013; Vlachou und Iakovidou, 2015.

Vorhandene Befragungsstudien (Delphi-Studien) sprechen tendenziell dafür, dass der starke steuerliche Fokus der ökonometrischen Literatur zu Steuern, Standortwahl und FDI womöglich insofern irreführend ist, als Steuern keinesfalls ein besonders bedeutsamer und herausragender Standortfaktor sind und womöglich strukturell von einigen Studien (die wenig auf andere Faktoren kontrollieren) überschätzt werden.

4.2.2. Indizes als Maßgrößen für Standortattraktivität

Als komplexes Konstrukt wird Standortattraktivität üblicherweise über Indizes gemessen, mit denen Ländervergleiche durchgeführt werden können. Diesen Indizes kommt damit eine ähnliche Bedeutung zu wie etwa dem *Human Development Index* der *UNO* oder dem *Corruption Perception Index* von *Transparency International.* Häufig finden Indizes auch Anwendung als Kontrollvariablen in der ökonomischen Literatur.[47]

Als derzeit international geläufigster Index zur Standortattraktivität lässt sich der Gesamtindex des *Global Competitiveness Report* des *World Economic Forum* (WEF) in Davos bezeichnen.[48] Darüber hinaus greifen wir auf den *Global Competitiveness Report*, das *World Competitiveness Yearbook* des *International Institute for Management Development* (IMD) und den *IW-Standortindex* des *Instituts für Weltwirtschaft* zurück. Nicht explizit berücksichtigt werden Indizes, deren Ziel nicht in der Messung von Standortattraktivität sondern von ökonomischer Freiheit liegt, deren Fokus auf speziellen Themen wie Bürokratieabbau liegt, oder deren Zielsetzung eher politischer Natur ist.[49]

Wir möchten darauf hinweisen, dass die verwendeten Indizes keine perfekten Maßgrößen für das Konstrukt Standortattraktivität darstellen. Insbesondere erscheinen die

[47] So verwenden etwa Barrios et al., 2012, S. 956f. sowie Mutti und Ohrn, 2019, S. 182 den *Economic Freedom of the World Index* des *Fraser Institute*, Hebous, Ruf und Weichenrieder, 2011, S. 838 den *Corruption Index* von *Transparency International* und Merlo, Riedel und Wamser, 2020, S. 43, den *Corruption Index* sowie den *Property Rights Index* der *Heritage Foundation.*

[48] Dies zeigt sich auch bei Untersuchungen mit Google Trends, in denen wir die von uns untersuchten Indizes miteinander vergleichen. Von den von uns untersuchten Indizes (inkl. des *Economic Freedom of the World Index* des *Fraser Institute* sowie des *Index of Economic Freedom* der *Heritage Foundation*) kommt der *Global Competitiveness Report* des WEF in den letzten 12 Monaten auf die meisten Suchanfragen; vgl. `https://trends.google.de/trends/explore?q=global%20competitiveness%20index,economic%20freedom%20of%20the%20world,world%20competitiveness%20yearbook,ease%20of%20doing%20business%20index,index%20of%20economic%20freedom` (07.09.2021).

[49] Da Steuern immer auch eine Einschränkung der wirtschaftlichen Freiheiten darstellen, ist davon auszugehen, dass Indizes zur Messung wirtschaftlicher Freiheiten die Relevanz von Steuern in Bezug auf die Wettbewerbsfähigkeit tendenziell überschätzen. Unberücksichtigt bleiben daher der *Economic Freedom of the World Index* des *Fraser Institute* (Gwartney et al., 2020, S. V, 3, 9) und der *Index of Economic Freedom* der *Heritage Foundation* (Heritage Foundation, 2021, S. 1-6, 455, 465). Ebenfalls nicht explizit berücksichtigt wird der *Ease of Doing Business Index* der *World Bank* (World Bank Group, 2020a, S.78ff.; World Bank Group 2020b, S. 4ff.). Dieser Index bildet insbesondere nicht die Breite der Standortfaktoren ab, sondern konzentriert sich sehr stark auf Themen wie Bürokratieabbau, Bürokratiekosten, zeitliche Verzögerungen bei Grundbuchämtern und vergleichbare Probleme. Das Ziel des *Länderindex Familienunternehmen* der *Stiftung Familienunternehmen* scheint vor allem darin zu liegen, Druck auf politische Entscheidungsträger aufzubauen, um Steuerbelastungen zu reduzieren (vgl. Stiftung Familienunternehmen, 2019, S. 6 sowie Fußnote 66).

Auswahl der berücksichtigten Standortfaktoren und deren Gewichtung im Gesamtindex als wenig transparent und methodisch angreifbar. So gewichtet etwa der *Global Competitiveness Report* jeden der 12 Subindizes gleich, ohne diese Gewichtung detailliert zu begründen. Auch sind die Bewertungen der einzelnen Indexkomponenten nicht im Detail nachvollziehbar. Letztlich handelt es sich bei den verwendeten Indizes um aggregierte Informationen von Expertengremien, die zahlreiche Einzelfaktoren und Gewichtungsentscheidungen von Experten explizit und in dieser Form auch transparent abbilden.

Der Index des *Global Competitiveness Report* des WEF gliedert sich in 12 Subindizes, die jeweils mit 8,3% in die Bewertung eingehen: (1) Institutionen, (2) Infrastruktur, (3) Adoption von Informations- und Kommunikationstechnologien (IuK-Technologien), (4) Makroökonomische Stabilität, (5) Gesundheit, (6) Fähigkeiten der Arbeitskräfte, (7) Produktmarkt, (8) Arbeitsmarkt, (9) Finanzsystem, (10) Marktgröße, (11) Geschäftsdynamik und (12) Innovationsfähigkeit. Steuerliche Aspekte werden im Rahmen der Subindizes (1), (7) und (8) als Belastung durch Bürokratiekosten sowie als Verzerrungen des Produktmarktes durch die Steuerbelastung von Unternehmen und des Arbeitsmarktes durch Steuern auf Löhne abgebildet. Berücksichtigt man ausschließlich Unternehmenssteuern, so ergibt sich ein Einfluss auf den Gesamtindex von nur 1,38%.[50] Aktuell belegt Deutschland Rang 7 von 141. Dies stellt gegenüber dem Vorjahr eine Verschlechterung um 4 Plätze dar, die vor allem auf die Bereiche Gesundheit (5), Produktmarkt (7) und Geschäftsdynamik (11) zurückzuführen ist.[51]

Stärken Deutschlands liegen in den Bereichen Innovationsfähigkeit (Rang 1), Makroökonomische Stabilität (Rang 1), Fähigkeiten der Arbeitskräfte (Rang 5), Marktgröße (Rang 5) sowie Infrastruktur (Rang 8). Schwächen weist Deutschland hingegen in den Bereichen Finanzsystem (Rang 25), Gesundheit (Rang 31) und Adoption von IuK-Technologien (Rang 36) auf.[52] Gerade der letzte Punkt weist auf die häufig bemängelten Schwächen Deutschlands in der Digitalisierung hin. Auch in den steuerlich relevanten Faktoren Bürokratiekosten (Rang 15), Verzerrung des Produktmarktes (Rang 19) sowie Steuerlast des Faktors Arbeit (Rang 99) ist die Performance eher mittelmäßig bis schwach. Besonders schwach (schlechter als Rang 50) schneidet Deutschland in den Unterkategorien „Organisierte Kriminalität“, „Regulierung von Interessenkonflikten“, „Mobilfunknetz“, „Glasfasernetz“, „Komplexität der Zölle“, „Lohn- und Arbeitsflexibilität“, „Bankengesundheit“ und „Kredit-Gap“ ab.[53] Bezüglich organisierter Kriminalität lässt sich festhalten, dass Deutschland bereits seit längerem als „Geldwäsche-Paradies“ in der

[50] Würden weiterhin die Steuerbelastung des Faktors Arbeit (1,04%) sowie die Bürokratiekosten durch Regulierung (0,35%) berücksichtigt, so ergibt sich ein maximaler direkter Einfluss der steuerlichen Faktoren auf den Gesamtindex von 2,77%; eigene Berechnungen auf Basis von WEF, 2019, S. 611ff.

[51] Vgl. WEF, 2018, S. 239f. und 2019, S. 238ff.

[52] Vgl. WEF, 2019, S. 238ff.

[53] Vgl. ebenda.

Europäischen Union mit einem geschätzten Volumen von 100 Mrd. € gewaschenen Geldes pro Jahr gilt.[54] Ein Sonderbericht verweist zudem auf Schwächen im Bildungssystem. Im Hinblick auf die Transformationsfähigkeit und die Herausforderungen der Pandemie findet sich Deutschland allerdings vor Frankreich oder den USA im oberen Mittelfeld (3. Dezil) wieder.[55]

Zu ähnlichen Ergebnissen kommen auch die anderen analysierten Indizes. Der Index des *World Competitiveness Yearbook* des *IMD* besteht aus 337 Einzelfaktoren in 4 Hauptkategorien (Wirtschaftliche Performance, Effizienz des Staates, Effizienz der Unternehmen, Infrastruktur). Dabei schneidet Deutschland in den folgenden Bereichen gut ab: Wissenschaftliche Infrastruktur (Rang 4), Beschäftigung (6), Gesundheit und Umwelt (6), Inländische Wirtschaft (6), Internationale Investitionen (7) und Internationaler Handel (8). Im Verhältnis zur Gesamtposition (Rang 17 von 63) eher durchschnittlich ist Deutschland in den folgenden Bereichen: Gesellschaftlicher Rahmen (14), Finanzen (17), Institutioneller Rahmen (18), Produktivität und Effizienz von Unternehmen (18), Öffentliche Finanzen (19), Arbeitsmarkt (20), Allgemeine Infrastruktur (21), Unternehmensgesetzgebung (23) und Technologische Infrastruktur (25). Schwächen weist Deutschland in Bildung (28), Managementpraktiken (32), Einstellungen und Werte von Unternehmen (43), Preise (39) und Steuerpolitik (58) auf.[56] Jeder Subindex (und damit auch der Subindex Steuerpolitik) geht mit 5% in den Gesamtindex ein. Allerdings berücksichtigt der Index nicht explizit den Anteil der Unternehmenssteuern. In Analogie zum Index des *World Economic Forum* (WEF) schätzen wir den Anteil der Unternehmenssteuerbelastung am Gesamteinfluss der Steuerpolitik auf 50% (neben der Steuerlast des Faktors Arbeit und den Bürokratiekosten).[57] Unter dieser Annahme tragen die Unternehmenssteuern direkt 2,5% zum Gesamtindex des *World Competitiveness Yearbook* bei.[58]

Im *IW-Standortindex* schneidet Deutschland gut in den Bereichen Wissen, Staat und Infrastruktur ab und belegt in der Gesamtwertung einen Spitzenplatz (Rang 3 von 45). Im Bereich Kosten, der als Indikatoren beispielsweise Steuer-, Arbeits-, Energie-, Zins- und Exportkosten beinhaltet, belegt es lediglich Platz 36 von 45. Zudem zeigt der Dynamikindex, der einen Vergleich der Standortqualität zwischen den Jahren 2000 und 2015 abbildet, dass Deutschland seine Position in diesem Bereich im Zeitverlauf eher verschlechtert hat. Wiederum kommt der Besteuerung bei der Berechnung des Gesamtindex eine geringe Bedeutung zu.[59]

[54] Vgl. Bussmann, 2015; Bussmann, 2016; Grantner, 2022.

[55] Vgl. WEF, 2020, S. 22, 51.

[56] Vgl. IMD, 2020b, S. 3.

[57] Vgl. eigene Berechnungen auf Basis von WEF, 2019, S. 611ff.

[58] Vgl. IMD, 2020a, S. 4; IMD, 2020b, S. 3.

[59] Vgl. Bähr und Millack, 2018, S. 9, 12ff.; Institut der deutschen Wirtschaft (2013), S. 242ff.

Tabelle 4.1 fasst die betrachteten Indizes und die direkte Bedeutung von Steuern zusammen und vergleicht diese mit dem Effekt von (öffentlichen und privaten) Investitionen im Allgemeinen sowie für spezielle Bereiche (etwa Infrastruktur).[60] Es lässt sich festhalten, dass der Unternehmenssteuerbelastung eine vergleichsweise geringe Bedeutung zukommt,[61] während sich Investitionen über unterschiedliche Kanäle (Forschung und Entwicklung, IuK-Technologien, Humankapital, Infrastruktur) deutlich stärker auf die erläuterten Indizes auswirken. Alle Indizes verweisen darauf, dass Deutschland trotz einer hohen Unternehmenssteuerlast ein attraktiver Standort ist, der in jüngster Zeit etwas an Beliebtheit verloren hat.[62] Dieser Rückgang in der Standortattraktivität ist vor allem durch Schwächen in den Bereichen Digitalisierung und (digitale) Infrastruktur bedingt.[63]

Table 4.1: Indizes zur Standortattraktivität, Unternehmenssteuern und Investitionen

Index	**Institution**	**Rang**	**Einfluss Steuern**	**Einfluss Investitionen**
Global Competitiveness Index	World Economic Forum (WEF)	7/141	1,38%	28,22%
World Competitiveness Yearbook	International Institute for Management Development (IMD)	17/63	2,50%	25,00%
IW-Standortindex	Institut der deutschen Wirtschaft	3/45	1,12%	23,21%

60 Die Investitionsanteile wurden wie folgt berechnet, wobei im Wesentlichen Sachkapital, Humankapital und Forschung und Entwicklung berücksichtigt werden. Beim *Global Competitiveness Index* werden die Kategorien „Infrastructure“, „ICT adoption“ und „Skills“ sowie die Unterkategorie „Research and development“ in der Kategorie „Innovation capability“ berücksichtigt (WEF 2019, S. 238-241). Für das *World Competitiveness Yearbook* werden die Unterpunkte "Domestic Economy”, “International Trade”, “International Investment”, “Productivity and Efficiency”, “Technological Infrastructure” und “Scientific Infrastructure” von Investitionen beeinflusst (IMD 2020b). Schließlich werden beim *IW-Standortindex* die Oberkategorien „Allgemeine Infrastruktur (ohne Überlebenraten der Bevölkerung)“, „Infrastruktur Luft/Bahn/Schiff“, „Humankapital (ohne Bevölkerungswachstum)“ sowie die Unterkategorien „Produktivität“, „Grad der Übernahme neuer Technologien“, „FuE-Ausgaben“, „Patente pro 1.000 Einwohner“, „Elektrizitätsverluste“ sowie „Energieverbrauch in Relation zum realen kaufkraftbereinigten BIP“ berücksichtigt (Institut der deutschen Wirtschaft Köln, 2013, S. 242ff.).

61 Im Jahresbericht *Economic Freedom of the World* (EFW) des *Fraser Institute* haben steuerliche Faktoren (*Top marginal tax rate*) einen Einfluss von 4% für den Gesamtindex. Deutschland belegt hier den Rang 21 von 162 (Gwartney et al., 2020, S. V, 3, 9). In den *Index of Economic Freedom* der *Heritage Foundation* gehen steuerliche Faktoren mit 8,33% ein. Deutschland belegt hier Platz 29 von 178 (Heritage Foundation, 2021, S. 1-6, 455, 465). Im *Ease of Doing Business Index* der *World Bank* gehen Steuern mit einem Gewicht von 2,44% ein und Deutschland belegt Rang 22 von 190 (World Bank Group, 2020a, S.78ff.; World Bank Group 2020b, S. 4ff).

62 Dafür sprechen auch der *Deloitte Global Manufacturing Competitiveness Index*, nach dem Deutschland im verarbeitenden Gewerbe global hinter China und den USA 2016 die Position 3 einnahm (Deloitte, 2016, S. 4), sowie das *EY Attractiveness Survey Europe*, nach dem Deutschland bei den ausländischen Direktinvestitionen derzeit auf dem 3. Rang in Europa liegt (Ernst Young, 2020, S. 44).

63 Im *IMD World Digital Competitiveness* Ranking belegt Deutschland insgesamt Platz 18, was vor allem auf Investitionsdefizite im Bereich Telekommunikation im Subindikator Technologie zurückzuführen ist. Unterdurchschnittlich sind zudem die Bereiche Breitbandausbau und Förderungen für technologische Entwicklung ausgeprägt (vgl. IMD, 2020b, S. 5-7).

Akzeptiert man die verwendeten Indizes trotz der erwähnten Schwächen als Maße für Standortattraktivität, sollte sich Deutschland zur Verbesserung der Attraktivität weniger auf die Senkung von Steuern, als auf die Beseitigung der in den Indizes dokumentierten Defizite im Bereich der Investitionen (Digitalisierung, Infrastruktur, Humankapital), aber auch in Bereichen wie Arbeitsmarkt, Bildungssystem, Rechtsstaat (organisierte Kriminalität) und Managementpraktiken konzentrieren. Dies ist dadurch bedingt, dass die erläuterten Bereiche ein größeres Gewicht für den Gesamtindex haben als die Besteuerung.

Dabei ist zu berücksichtigen, dass die Steuerpolitik auch für diese nichtsteuerlichen Bereiche relevant ist. Erstens lässt sich durch Unternehmenssteuern Aufkommen erzielen, das für Qualitätsverbesserungen in den identifizierten Bereichen verwendet werden kann. Da Steuersenkungen öffentliche Einnahmen reduzieren, schränken sie den finanziellen Spielraum des Staates ein.[64] Zweitens lassen sich durch steuerpolitische Maßnahmen (etwa Sonderabschreibungen und Subventionen) Anreize für zusätzliche private Investitionen in Bereichen wie (digitale) Infrastruktur, Forschung und Entwicklung, Humankapital, Nachhaltigkeit oder gar zur Verbesserung von Corporate Governance-Strukturen setzen. Drittens können steuerpolitische Anreize auch zu Fehlallokationen von Kapital führen. So stellt sich etwa die Frage, ob die extrem großzügigen Begünstigungen für Investitionen in Bestandsimmobilien in Deutschland (effektive Steuerfreiheit des Veräußerungsgewinns nach § 23 EStG in Verbindung mit Abschreibungen bei den Einkünften aus Vermietung und Verpachtung gem. § 21 EStG) nicht zu Überinvestitionen in Bestandsimmobilien und zu Unterinvestitionen in anderen Bereichen führen.[65] Viertens dürften hohe Steuer- und Abgabebelastungen auf den Faktor Arbeit dazu führen, dass Arbeitskosten steigen und das Arbeitsangebot sinkt. Hohe Abgaben auf den Faktor Arbeit können auch dadurch (mit)bedingt sein, dass andere Einkünfte weniger stark besteuert werden. Fünftens dürfte dem Steuersystem als Teil des Rechtstaates durchaus auch eine disziplinierende Rolle bei der Bekämpfung von Kriminalität und Geldwäsche zukommen. Desai und Dharmapala (2006) finden Belege, dass mehr Steuervermeidung nur dann zu höheren Unternehmenswerten führt, wenn die Corporate Governance ausreichend hoch ist, um nicht von Managern oder einflussreichen Aktionären vereinnahmt zu werden.

Abschließend lässt sich festhalten, dass es nach unserer Kenntnis nur einen Index zur Standortattraktivität gibt, der dem Thema Besteuerung eine hohe Bedeutung zuweist. Da das Ziel dieses von der Lobbyorganisation *Stiftung Familienunternehmen* herausgegebenen Index primär darin bestehen dürfte, den deutschen Politikbetrieb im Sinne deutscher

[64]Dabei nehmen wir an, dass sich Deutschland in einem Bereich der „Lafferkurve“ befindet, in dem Steuersatzsenkungen zu weniger Einnahmen führen. Dies dürfte bei Unternehmenssteuersätzen von etwa 30% weitgehend unstrittig sein. So finden etwa Trabandt und Uhlig (2011), S. 318, erst ab Steuersätzen auf Kapital von über 60%, dass eine Steuersatzerhöhung zu einer Reduktion des Gesamtaufkommens führt. Auch die von ihnen zitierte Literatur kommt zu ähnlichen Ergebnissen.

[65]Vgl. hierzu auch Bach und Eichfelder (2021), S. 2938f.

Familienunternehmer zu beeinflussen, wird er im Folgenden vernachlässigt.[66]

4.3. Indirekte Effekte auf die Standortattraktivität über Investitionen

Die Erläuterungen des Kapitels 4.2 und insbesondere die Analyse der Standortindizes in 4.2.2 machen deutlich, dass der direkte Einfluss der Unternehmensbesteuerung auf die Standortattraktivität eher moderat ist und anderen Standortfaktoren eine größere Bedeutung zukommt. Im Modell von Devereux und Griffith (2003) lässt sich dies implizit dadurch berücksichtigen, dass die Produktionsfunktion von zahlreichen anderen Faktoren abhängig ist, deren Einfluss auf das Ergebnis deutlich stärker ist als der Einfluss der Besteuerung. Ein entsprechendes Modell legt nahe, dass sich politische Entscheider zur Verbesserung der Standortattraktivität vor allem auf die wichtigsten Faktoren der Standortwahl konzentrieren sollten, zu denen Steuern anscheinend nicht gehören. Im folgenden Abschnitt soll nun der Frage nachgegangen werden, ob steuerpolitische Maßnahmen dazu geeignet sind, nichtsteuerliche Standortfaktoren zu verbessern (indirekte Effekte). Dabei zielt unsere Argumentation insbesondere auf den Einfluss von Investitionen ab, die sich entsprechend Tabelle 1 als wichtige Treiber der Standortattraktivität charakterisieren lassen. Primär diskutieren wir dabei den Einfluss der Unternehmensbesteuerung (inkl. steuerlichen Anreizen) auf private Investitionen. Es ist allerdings auch zu berücksichtigen, dass nichtgewährte Steuervergünstigungen bzw. Steuererhöhungen auch dazu genutzt werden, öffentliche Investitionen zu tätigen, die ebenfalls die Standortattraktivität verbessern können.

Wir konzentrieren uns zunächst auf die empirische Literatur, die den Einfluss der Kapitalnutzungskosten (*user costs of capital*) auf die Investitionstätigkeit oder den Kapitalstock von Unternehmen (also nicht nur auf ausländische Direktinvestitionen) untersucht. Dabei werden Steuern entsprechend der neoklassischen Investitionstheorie[67] als

[66] Das Ziel des *Länderindex Familienunternehmen* scheint vor allem darin zu liegen, Druck auf politische Entscheidungsträger aufzubauen, um Steuerbelastungen zu reduzieren. Er besteht aus sechs Subindizes, wobei der Subindex Steuern mit einer Gewichtung von 20% den stärksten Einfluss auf das gesamte Ranking hat (anders also als etwa Unternehmensbefragungen regelmäßig suggerieren). Innerhalb dieser Kategorie nimmt die „Steuerbelastung im Erbfall“ mit 30% den höchsten Stellenwert ein (vgl. Stiftung Familienunternehmen, 2019, S. 6), obwohl die Erbschaftsteuer im Verhältnis zu Unternehmenssteuern kein relevantes Aufkommen erzielt und Erbschaften von Betriebsvermögen in Deutschland einer steuerlichen Begünstigung von bis zu 100% unterliegen (vgl. Watrin und Linnemann, 2017 mit weiteren Nachweisen). Im Jahr 2018 wurde durch die Erbschaftsteuer ein Aufkommen von 6,8 Mrd. € erzielt, während das Aufkommen der Gewerbesteuer bei 55,8 Mrd. €, der Körperschaftsteuer bei 33,4 Mrd. € und der veranlagten Einkommensteuer bei 60,4 Mrd. € liegt (vgl. Statistisches Bundesamt, 2020, S. 281). Bereits aus dieser Aufstellung wird deutlich, dass der Erbschaftsteuer im Verhältnis zu den Ertragsteuern keine nennenswerte Bedeutung zukommt. Deutschland liegt nach dieser Methodik auf Platz 16 von 21, was vor allem durch das schlechte Abschneiden im Bereich Steuern bedingt ist (vgl. Stiftung Familienunternehmen, 2019, S. 93ff.).

[67] Vgl. etwa Hall und Jorgenson, 1967.

eine Komponente der Kapitalnutzungskosten interpretiert. Empirische Studien in dieser Tradition finden Elastizitäten zwischen den Kapitalnutzungskosten und dem Kapitalstock von Unternehmen zwischen -0,2 und -1.[68] Diese Bandbreite erlaubt allerdings noch keinen Rückschluss auf die Semi-Elastizität zwischen dem Steuersatz und der Investitionstätigkeit. Eine entsprechende Semi-Elastizität lässt sich anhand von einfachen theoretischen Überlegungen berechnen. Die Kapitalnutzungskosten C lassen sich schreiben als[69]

$$C_t = \varphi_t \cdot T_t \cdot [\rho_t + \delta_t - E(\Delta\varphi_t/\varphi_t)]. \tag{4.2}$$

Dabei bezeichnet φ_t das Preisniveau zum Zeitpunkt t, ρ_t die Opportunitätskosten des eingesetzten Kapitals nach Steuern (gemischter Kostensatz für Eigen- und Fremdkapital), T_t den Steuerterm, δ_t die Bruttorate der realen Abschreibung und $E(\Delta\varphi_t/\varphi_t)$ die erwartete Veränderung des Preisniveaus. Somit bezeichnet $\delta_t - E(\Delta\varphi_t/\varphi_t)$ die erwartete Nettorate der Abschreibung. Deutlich wird, dass eine Investition unter Vernachlässigung von Preisniveau und Steuern die Summe aus den Opportunitätskosten des eingesetzten Kapitals und der realen ökonomischen Abschreibungsrate erwirtschaften muss, um rentabel zu sein. Der Steuerterm T_t lässt sich schreiben als[70]

$$T_t = \frac{(1 - \tau_t \cdot Z_t - s_t)}{(1 - \tau_t)}. \tag{4.3}$$

Dabei bezeichnen s_t die Rate der Direktsubventionen für Investitionen (im Regelfall 0%), τ_t den nominellen Steuersatz für Unternehmen und Z_t den auf eine Investitionssumme von 1 normierten Barwert der Abschreibungen. Im Falle einer Sofortabschreibung nimmt Z_t den Wert 1 an. Es wird deutlich, dass in diesem Fall für $s_t = 0$ Steuern keinen Einfluss auf die Kapitalnutzungskosten haben. Steuern sind also nur dann relevant, wenn der Barwert der Abschreibungen geringer ist als 1. Dieser hängt wiederum von den Kapitalkosten nach Steuern sowie der Abschreibungsdauer ab.

Unterstellt man Opportunitätskosten des Kapitals von 7,5%,[71] eine Abschreibungsdauer von durchschnittlich 7 Jahren für Industrieanlagen[72] und berücksichtigt, dass die bisherigen Schätzungen insbesondere auf US-Daten vor der *US Tax Reform* 2018 beruhen,[73]

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[68] Vgl. Auerbach und Hassett, 1992, S.151f.; Cummins, Hassett und Hubbard, 1994, S. 43; Caballero, Engel und Haltiwanger, 1995, S. 4; Chirinko, Fazzari und Meyer, 1999, S. 56; Bond und Van Reenen, 2007; Schwellnus und Arnold, 2008, S. 10; Dwenger, 2014, S. 161; Bond und Xing, 2015, S. 27; Mutti und Ohrn, 2019, S. 166; Melo-Becerra, Mahecha und Ramos-Forero, 2021, S. 3.

[69] Vgl. hierzu etwa Auerbach, 1983; Cohen, Hansen und Hassett, 2002, S. 459f.; Dwenger, 2014, S. 163.

[70] Vgl. Cohen, Hansen und Hassett, 2002, S. 459f.

[71] Vgl. ebenda.

[72] Vgl. Spengel et al., 2019, S. A-24.

[73] Vgl. hierzu auch Auerbach und Hassett, 1992; Chirinko, Fazzari und Meyer, 1999; Bond und Xing, 2015. Wir unterstellen daher einen Unternehmenssteuersatz von vereinfachend 40%. Dieser beruht

dann führt eine Veränderung des Unternehmenssteuersatzes um einen Prozentpunkt zu einer Veränderung der Kapitalnutzungskosten von 0,59 Prozent.[74] Bei kurzen bis mittleren Abschreibungszeiträumen bedeutet also eine Elastizität zwischen Kapitalnutzungskosten und Investitionen von -0,2 und -1 eine Semi-Elastizität zwischen Unternehmenssteuersatz und Investitionen von -0,12 bis -0,59. Dementsprechend würde eine Erhöhung des Unternehmenssteuersatzes um einen Prozentpunkt ceteris paribus (d.h. wenn etwa die staatlichen Ausgaben konstant gehalten werden) zu einem Rückgang der Investitionstätigkeit von nur 0,12% bis 0,59% führen. Dies dokumentiert – in Übereinstimmung mit einer theoretischen Simulationsstudie des Ifo-Institutes[75] – einen nur moderaten Einfluss des Steuersatzes von Unternehmen auf deren Investitionstätigkeit.

Deutlich höhere Elastizitäten von -3,7 bis -14 ermitteln demgegenüber Studien, die sich mit dem Einfluss von zeitlich beschränkten Investitionsanreizen durch Sonderabschreibungen auf Investitionen beschäftigen.[76] Diese Elastizitäten werden auf Basis des Steuerterms der Kapitalnutzungskosten in Formel 4.3 geschätzt und beschreiben damit die Änderung des Investitionsvolumens im Verhältnis der durch die Sonderabschreibung bedingten Minderung der Kapitalnutzungskosten.[77] Auch dies lässt sich anhand eines Beispiels konkretisieren. So führt bei analogen Annahmen zu oben (Opportunitätskosten des Kapitals 7,5%, Steuersatz 40%, reguläre Abschreibungsdauer 7 Jahren) die Einführung einer 100%igen Sonderabschreibung zu einer Zunahme des Barwertes der Abschreibungssumme von 75,7% auf 93,0% der Ausgangsinvestition[78] und zu einer erheblichen Abnahme der Kapitalnutzungskosten von 10%. Bei einer Elastizität von -3,7 impliziert dies näherungsweise eine starke Zunahme der Investitionen um 37% (bei exakter

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auf einer *Federal Corporate Income Tax* von 35% sowie weiteren Unternehmenssteuern (vor allem *State Income Taxes*) von etwa 5%; vgl. Bundesministerium der Finanzen, 2018, S. 17.

[74]Unter der Annahme, dass Direktsubventionen nicht vorhanden sind, lässt sich der Steuerterm der *User Costs of Capital* schreiben als $T_t = \frac{(1-\tau_t \cdot Z_t)}{(1-\tau_t)}$. Setzt man für den Steuersatz 40% ein und ermittelt den auf maximal 1 normierten Barwert der Abschreibungsvorteile als $Z_t = \frac{1}{D} \cdot \frac{(1+r)^D-1}{(1+r)^D \cdot r}$ mit den Kapitalkosten $r = 7{,}5\%$ und der Abschreibungsdauer $D = 7$ Jahre, dann ermittelt sich ein Barwert der Abschreibungen in Höhe von 75,66% des Investitionsvolumens und ein Steuerterm von 1,1622. Dies bedeutet, dass Unternehmenssteuern die Kapitalnutzngskosten um 16,22% steigern. Wir führen eine analoge Berechnung mit einem Steuersatz von 41% durch und ermitteln einen Wert von 1,1691. Ein Vergleich beider Werte macht deutlich, dass bei einem Anstieg des Steuersatzes um einen Prozentpunkt die Kapitalnutzungskosten um 0,59% zunehmen. Dieser nichtproportionale Zuwachs ist darauf zurückzuführen, dass die Abschreibungsvorteile einen Teil der Steuerlast auf die Cash Flows kompensieren.

[75]Vgl. Dorn et al., 2021, S. 8.

[76]Vgl. House und Shapiro, 2008, S. 737; Zwick und Mahon, 2017, S. 218; Ohrn, 2018, S. 272; Eichfelder und Schneider, 2018, S. 24f.; Maffini, Xing und Devereux, 2019, S. 372; Guceri und Albinowski, 2021, S. 1148f.

[77]Wie sowohl Formel (3) als auch unsere Erläuterungen in Fn. 74 verdeutlichen, hängt der Einfluss der Sonderabschreibungen auf die Kapitalnutzungskosten nicht nur von der Minderung der Abschreibungsdauer im Verhältnis zu den regulären Abschreibungen ab, sondern auch vom Steuersatz τ_t und der Rendite der Alternativanlage nach Steuern r_t.

[78]Vgl. hierzu auch Fn. 74. Da die Sonderabschreibung eine Periode verzögert eintritt, ist der normierte Wert dieser Abschreibung $1/1{,}075 = 0{,}9302$. Daraus ermittelt sich ein Steuerterm von 1,047, was deutlich unter dem Steuerterm von 1,1622 bei einer 7-jährigen Abschreibung liegt.

Berechnung mit der e-Funktion 44,8%).

Die Bandbreite der Elastizitäten von -3,7 bis -14 lässt sich gut mit der Bandbreite von -0,2 bis -1,0[79] von Studien vergleichen, die ebenfalls auf Basis der Kapitalnutzungskosten den Einfluss von (permanenten) Steuersatzänderungen untersuchen. Ursächlich für den deutlich stärkeren Einfluss von temporären Sonderabschreibungen auf Investitionen dürften drei Aspekte sein. Zum einen führen steuerliche Investitionsanreize nur dann zu einer Verminderung der Steuerlast, wenn Investitionen getätigt werden. Sie sind also das zielgenauere Instrument und schaffen einen expliziten Anreiz, Realinvestitionen zu tätigen und die freien Mittel nicht beispielsweise über Aktienrückkäufe und Dividenden an Aktionäre auszuschütten.[80] Sie führen auch nicht zu Mitnahmeeffekten für Unternehmen, die nicht mit Investitionen auf die Anreize reagieren. Zum zweiten führen Sonderabschreibungen genau dann zu einem Anstieg der Liquidität, wenn auch Investitionen getätigt werden und der Kapitalbedarf entsprechend hoch ist. Der Liquiditätszufluss erfolgt regelmäßig in der Folgeperiode, in der die Steuererklärung für die Investitionsperiode eingereicht wird. Soweit positive Gewinne erzielt werden, dürften davon insbesondere Unternehmen mit Liquiditätsproblemen profitieren. Dies findet auch Bestätigung durch die empirische Literatur.[81] Demgegenüber profitieren von Steuersatzsenkungen vor allem Unternehmen mit hohen Gewinnen, unabhängig davon, ob diese Investitionen tätigen oder nicht, wobei Steuersatzsenkungen den Effekt von Abschreibungen zusätzlich reduzieren. Zum dritten führt gerade die zeitliche Beschränkung eines Investitionsanreizes dazu, dass dieser noch verstärkt wird, da nur Unternehmen profitieren, die schnell auf die Förderung reagieren. Als Konsequenz werden Investitionen vorgezogen, was den kurzfristigen Effekt deutlich verstärkt und diese zu einem klassischen Instrument der antizyklischen Konjunkturpolitik macht. Dies impliziert allerdings auch, dass ein Teil des Effektes auf eine zeitliche Verlagerung von Investitionen in die Förderperiode und damit nicht auf eine langfristige Steigerung des Kapitalstockes zurückzuführen ist. Die empirische Literatur liefert zahlreiche Belege, dass gerade entsprechende temporäre Anreize zur Investitionsförderung die Investitionstätigkeit erheblich steigern können. Allerdings liefert sie keine stichhaltigen Belege, welcher Teil des Effektes sich auf eine rein temporäre Verlagerung

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[79] Vgl. Auerbach und Hassett, 1992, S.151f.; Cummins, Hassett und Hubbard, 1994, S. 43; Caballero, Engel und Haltiwanger, 1995, S. 4; Chirinko, Fazzari und Meyer, 1999, S. 56; Bond und Van Reenen, 2007; Schwellnus und Arnold, 2008, S. 10; Dwenger, 2014, S. 161; Bond und Xing, 2015, S. 27; Mutti und Ohrn, 2019, S. 166; Melo-Becerra, Mahecha und Ramos-Forero, 2021, S. 3.

[80] Yagan, 2015, S. 3531f. findet empirische Belege, dass eine starke Kürzung der Dividendenbesteuerung in den USA nicht zu einer Erhöhung von Investitionen, sondern zu einer Erhöhung von Dividenden und von Aktienrückkäufen geführt hat.

[81] Edgerton (2010), S. 936ff. findet Belege, dass Verlustunternehmen schwächer auf Sonderabschreibungen reagieren und Guceri und Albinowksi (2021), S. 1147 finden entsprechende Evidenz für Unternehmen mit hohen Risiken. Zwick und Mahon (2017), S. 217ff. finden Belege, dass Unternehmen mit Liquiditätsproblemen stärker auf Sonderabschreibungen reagieren, während Maffini, Xing und Devereux (2019), S. 384 keine unterschiedlichen Reaktionen für Unternehmen mit hohen oder geringen Cash Flows feststellen können.

von Investitionen und nicht auf eine dauerhafte Erhöhung des Kapitalstockes zurückführen lässt.[82]

Dies gilt auch für Krisenzeiten, da die von Seiten der empirischen Literatur untersuchten Investitionsanreize häufig als antizyklische Fördermaßnahmen konzipiert wurden (etwa Sonderabschreibungen zur Bekämpfung der Wirtschaftskrise 2001 oder zur Bekämpfung der Finanzkrise 2008).[83] Allerdings lässt sich festhalten, dass gerade in Krisenzeiten Unternehmen, die Verluste machen oder stärker von Unsicherheit betroffen sind, weniger stark auf Investitionsanreize in Form von Sonderabschreibungen reagieren dürften. Dies ist dadurch bedingt, dass a) Verlustunternehmen nur dann direkte Liquiditätsvorteile durch Sonderabschreibungen haben, wenn ein Verlustrücktrag möglich ist und b) Unternehmen bei hohen Risiken dazu tendieren, Investitionen in die Zukunft zu verlagern. Der Effekt b) lässt sich dadurch reduzieren, dass Sonderabschreibungen nur temporär gewährt werden und damit Zeitdruck aufgebaut wird.

Aus Perspektive des Fiskus stellen steuerliche Abschreibungsvorteile keinen Verzicht sondern nur eine Stundung von Steueransprüchen dar. Da sich der Staat kostengünstiger verschulden kann als private Unternehmen, zieht eine derartige Steuerstundung nur geringe bis moderate Kostenbelastungen für die öffentlichen Haushalte nach sich.[84] Demgegenüber würde eine dauerhafte Senkung des Steuersatzes von Unternehmen das Finanzierungsvolumen der öffentlichen Hand nachhaltig mindern.

Sowohl allgemeine Senkungen der Unternehmenssteuerbelastung als auch (zeitlich begrenzte) Investitionsfördermaßnahmen mindern ceteris paribus die Ressourcen, die für öffentliche Direktinvestitionen zur Verfügung stehen. Dementsprechend erscheint auch ein Blick auf die Literatur, die sich mit den Auswirkungen derartiger Investitionen auf das Wachstum und die Attraktivität von Volkswirtschaften beschäftigt, sinnvoll. Eine Meta-Studie von Bom und Ligthart (2008) schätzt die durchschnittliche Output-Elastizität von Infrastrukturkapital auf 0,08%.[85] Bezogen auf Deutschland würde das bedeuten, dass eine öffentliche Investition in die Infrastruktur i.H.v. 45 Mrd. € das jährliche Bruttoinlandsprodukt um 11,25 Mrd. € steigern würde, was einer impliziten Rendite von 25% entspricht.[86] Auch wenn diese Rendite angesichts öffentlichkeitswirksamer Skandale wie

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[82]Vgl. Müller, 2000, S. 201f.; House und Shapiro, 2008, S.737; Zwick und Mahon, 2017, S.218; Eichfelder und Schneider, 2018, S. 24f.; Kompolsek, Riedle und Ruf, 2018, S.17; Maffini, Xing und Devereux, 2019, S. 372.

[83]Vgl. House und Shapiro, 2008, S.737; Zwick und Mahon, 2017, S.218; Maffini, Xing und Devereux, 2019, S. 372.

[84]An dieser Stelle ließe sich einwenden, dass auch Unternehmen von den geringen Zinssätzen profitieren, was den steuerlichen Fördereffekt von Sonderabschreibungen mindert. Aufgrund höherer Risiken sind allerdings die Kapitalkosten von Unternehmen (Eigen- und Fremdkapital) derzeit um ein Vielfaches höher als die Kapitalkosten des Staates; vgl. zu Kapitalkosten des Eigenkapitals etwa Botosan, Plumlee, und Wen, 2011.

[85]Vgl. Bom und Ligthart, 2008, S. 23.

[86]Vgl. IMK, 2019, S. 7-10.

dem BER oder der Elbphilharmonie hoch erscheint, deuten weitere Studien auf ähnlich hohe Elastizitäten hin. Dabei dürfte das durchschnittliche Investitionsprojekt nicht in Prestigebauten sondern in der lokalen Schule oder dem lokalen Stromnetz bestehen.[87] Der Sachverständigenrat zur Begutachtung der Gesamtwirtschaftlichen Entwicklung hat im Jahr 2003 eine Output-Elastizität von 0,29 gemessen. Bezogen auf Deutschland könnte demnach eine 1-prozentige Steigerung der Investitionsquote der Unternehmen bzw. der staatlichen Infrastrukturinvestitionen das BIP jeweils um 2,4% bzw. 0,1% erhöhen. Letzteres würde zusätzlich zu um 0,08% erhöhten privaten Investitionen führen und den Effekt weiter verstärken.[88] Diese Befunde werden durch eine Metastudie von Gechert (2015) bekräftigt, die untersucht, welche Art von Fiskalpolitik den stärksten Multiplikationseffekt hinsichtlich der Gesamtwirtschaftsleistung zeigt. Der effektivste staatliche Impuls lässt sich demnach bei den öffentlichen Investitionen beobachten, wobei in Krisenzeiten stärkere Effekte zu beobachten sind.[89] Diese Befunde bestärken unsere Argumentation, dass temporäre Sonderabschreibungen geeignetere Investitionsanreize sind als dauerhafte Steuersatzsenkungen, da diese zielgerichtet und direkt Investitionen stimulieren und gleichzeitig staatliche Ressourcen schonen, die für Direktinvestitionen mit dem Ziel der Verbesserung der Standortattraktivität in Bereichen wie Digitalisierung (Verwaltung, Bildungssystem), Infrastruktur oder Humankapital (Bildungssystem) genutzt werden können.

4.4. Diskussion und Fazit

Verbände wie der BDI fordern seit geraumer Zeit eine erhebliche Senkung der laufenden Steuerbelastung von Personen- und Kapitalgesellschaften, um die Attraktivität des Wirtschaftsstandortes Deutschland zu stärken und im Steuerwettbewerb mit anderen Nationen nicht den Anschluss zu verlieren. Diese Diskussion ist zum Teil auch durch die erhebliche Reduktion der Unternehmenssteuerbelastung in den USA bedingt, die im Jahr 2018 die Bundessteuer für Kapitalgesellschaften (*Federal Corporate Income Tax*) von 35% auf 21% gesenkt haben. Es bleibt allerdings abzuwarten, wie nachhaltig die Senkung der Unternehmenssteuern in den USA ist. In Großbritannien ist inzwischen wieder eine Anhebung der Unternehmenssteuern von 19% auf 25% geplant.[90]

Verwendet man international übliche Indizes zur Messung der Standortattraktivität als Maßgröße, dann dürfte der direkte Einfluss der Unternehmensbesteuerung auf die Attraktivität eines Wirtschaftsstandorts eher gering sein. Gemäß dem *Global Competitiveness Index* (GCI) des WEF liegt der Einfluss der Steuerlast von Unternehmen auf den

[87]Vgl. Van Suntum et al., 2008, S. 9ff. mit weiteren Nachweisen.
[88]Vgl. Sachverständigenrate zur Begutachtung der gesamtwirtschaftlichen Entwicklung, 2003, S. 324ff.
[89]Vgl. Gechert, 2015, S. 567ff.
[90]Vgl. `https://www.gov.uk/guidance/corporate-tax` (02.09.2021).

Gesamtindex (ohne steuerliche Bürokratiekosten) nur bei 1,38%. Im IW-Standortindex beträgt der Einfluss der Besteuerung 1,12%. Daher argumentieren Bähr und Millack (2018), dass der schlechte Rang von Deutschland im Bereich Kosten (36 von 45) vor allem auf die hohen Arbeitskosten zurückzuführen sei.[91] Gleiches gilt für das *World Competitiveness Yearbook* des IMD, bei dem die Steuerbelastung der Unternehmen für 2,50% des Index verantwortlich ist.[92]

Die Erläuterungen in Kapitel 4.2 deuten darauf hin, dass Deutschland erhebliche Defizite in den Bereichen Digitalisierung, Bildung, (digitale) Infrastruktur und Arbeitsmarkt aufweist, die wiederum eine hohe Bedeutung für die Standortattraktivität haben.[93] Im Bereich Infrastruktur bestehen besondere Schwächen in der Qualität der Straßeninfrastruktur sowie Effizienz des Lufttransportes. Schwächen bestehen darüber hinaus auch in Bereichen wie Arbeitskräfteangebot (Demographie), Bildung (Digitalisierung), Corporate Governance (Wirecard) und Managementpraktiken, Energiekosten, Finanzsystem, Gesundheit, Lohn- und Arbeitsflexibilität und organisierte Kriminalität (etwa Geldwäsche).[94] Es liegt nahe, Steuern als ein Instrument aufzufassen, um bereichsspezifische Schwächen Deutschlands anzugehen.

Ein passendes Instrument, um gezielt Anreize für Investitionen in bestimmten Bereichen zu schaffen sind steuerliche Sonderabschreibungen. Die in Kapitel 3 erläuterte empirische Literatur liefert eindrucksvolle Belege, dass gezielte und zeitlich beschränkte Investitionsförderprogramme effektiver Investitionsaktivitäten anregen als dauerhafte Senkungen der Unternehmenssteuersätze. Vor diesem Hintergrund erscheint es unverständlich, dass die letzte Koalitionsregierung unter Kanzlerin Merkel in der Covid-19-Krise keine antizyklischen Sonderabschreibungsprogramme aufgelegt hat, um mehr Investitionen in Bereichen wie Digitalisierung, Infrastruktur, Elektromobilität und Netzausbau sowie Dekarbonisierung anzuregen. Allerdings sieht der Koalitionsvertrag der Regierung Scholz umfassende Sonderabschreibungsprogramme (sogenannte „Superabschreibung“) in den Bereichen Klimaschutz und Digitalisierung vor, die durchaus noch einen positiven Beitrag zur Entwicklung des Wirtschaftsstandortes Deutschland leisten können.[95]

Im Rahmen einer künftigen Umsetzung dieser Programme sollten bekannte Schwachstellen von Konjunkturprogrammen beachtet werden. Steuervergünstigungen oder Subventionen in Form von Sonderabschreibungen sollten so gestaltet werden, dass Überinvestitionen bzw. eine ineffiziente Verwendung der öffentlichen Mittel verhindert und

[91] Vgl. Bähr und Millack, 2018, S. 12f.

[92] Eigene Berechnungen auf Basis von World Bank Group, 2020, S.78ff.

[93] Dabei gehen alleine die Subindizes „Infrastruktur“ und „Adoption von IuK-Technologien“ etwa zu jeweils 8,3% in den Gesamtindex des *Global Competitiveness Report* ein; vgl. etwa WEF, 2019, S. 238ff., 611ff.

[94] Vgl. hierzu detailliert unsere Ausführungen in Kapitel 4.3

[95] Vgl. SPD, BÜNDNIS 90/DIE GRÜNEN, FDP (2021), S. 164.

Mitnahme- und Substitutionseffekte möglichst minimiert werden. So finden Eichfelder, Jacob und Schneider (2020) Evidenz, dass die Einführung von Sonderabschreibungen in den 5 neuen Bundesländern zu einer Verminderung der durchschnittlichen Qualität von Unternehmensinvestitionen geführt hat.[96] Auch sollten Spillover-Effekte in andere Bereiche berücksichtigt werden. Während positive Spillover-Effekte bei Anreizen für Investitionen in (digitale) Infrastruktur zu erwarten sind, könnten einzelne Branchen, die neue Technologien noch nicht adoptiert haben, zumindest zunächst negativ beeinflusst werden (z.B. im Bereich der konventionellen Autoindustrie bei Elektromobilität oder im Bereich des klassischen Einzelhandels bei E-Commerce). Die besprochene empirische Literatur findet allerdings die starken positiven Effekte von Investitionsanreizen auf Investitionsvolumina regelmäßig im Aggregat,[97] sodass insgesamt von positiven Effekten auf die Investitionstätigkeit auszugehen ist.[98]

Außerdem ist zu beachten, dass die Covid-19-Krise wie auch der Krieg in der Ukraine zu einer Kombination aus einem Angebotsschock und einem zeitgleich auftretenden Nachfrageschock geführt haben. Es muss daher zusätzlich sichergestellt werden, dass inländische Unternehmen den eigenen Kapitalstock ausreichend auslasten können und Verzögerungen in der Produktion nicht dazu führen, dass entstehende Nachfragelücken angebotsseitig nicht bedient werden.[99]

Zusammenfassend lässt sich feststellen, dass eine reine Senkung der Unternehmenssteuerlast in Deutschland kein geeignetes Instrument zur nachhaltigen Verbesserung der Standortattraktivität darstellt. Insgesamt ist Deutschland als eine der stärksten und wirtschaftlich attraktivsten Volkswirtschaften auf dem europäischen Kontinent einzustufen. Im Verhältnis zu den europäischen Wettbewerbern impliziert dies im Sinne der Theorie der Neuen Ökonomischen Geographie hohe Steuersätze. Eine Senkung von Unternehmenssteuersätzen dürfte insbesondere europäische Wettbewerber unter Druck setzen und damit eine weitere Runde im Steuersenkungswettlauf auslösen.[100]

Allerdings sprechen Defizite Deutschlands in einzelnen Teilbereichen für eine punktuelle Förderung von Investitionen. Hier bieten sich neben den hier propagierten Sonderabschreibungen auch verstärkte öffentliche Investitionen an. Nach einem Bericht des Instituts für Makroökonomie und Konjunkturforschung (IMK) (2019) bestehen bereits seit geraumer Zeit massive Investitionslücken im Bereich der öffentlichen Infrastruktur. Trotz

[96] Vgl. etwa Müller, 2000, S. 204; Eichfelder, Jacob und Schneider, 2020.

[97] Vgl. etwa House und Shapiro, 2008, S.737; Zwick und Mahon, 2017, S.218; Eichfelder und Schneider, 2018, S. 24f.; Maffini, Xing und Devereux, 2019, S. 372.

[98] Der *Global Competitiveness Report* 2020 betont zudem die Transformationsnotwendigkeit der betrachteten Volkswirtschaften. Es ist zu erwarten, dass die Wichtigkeit der sog. „Mega-Trends“ für die Standortattraktivität und Wettbewerbsfähigkeit in Zukunft daher eher weiter zunehmen wird (WEF, 2020).

[99] Vgl. Grömling et al., 2020, S. 7.

[100] Vgl. etwa Eichfelder, 2018 mit weiteren Nachweisen.

der extrem günstigen Rahmenbedingungen, charakterisiert durch hohe Steuereinnahmen bei gleichzeitig geringen Zinssätzen, sehen die Autoren eine Investitionslücke i.H.v. 450 Mrd. € für die nächsten 10 Jahre.[101] Ein solcher „Investitionsstau“ verhindert nicht nur die optimale Entwicklung des öffentlichen Kapitalstocks, sondern beeinträchtigt auch Geschäftsabläufe und damit die Attraktivität des Wirtschaftsstandortes, die seit geraumer Zeit moniert werden. Bereits Untersuchungen des BDI sowie des Instituts der deutschen Wirtschaft (IW) aus den 2000er Jahren beklagen den „Investitionsstau“ im Ausbau der Infrastruktur in den Bereichen Verkehr, Energie und Telekommunikation,[102] wobei Bedenken geäußert wurden, die Infrastruktur könne sich „von einem Wachstumsmotor zu einer Wachstumsbremse entwickeln“.[103] Dies verdeutlichen auch Studien des IW aus den Jahren 2013 und 2018. Demnach hat die Zahl der Unternehmen, die sich durch Infrastrukturmängel in ihrer Geschäftstätigkeit beeinträchtigt fühlen, von 2013 auf 2018 um 10 Prozentpunkte auf 68% zugenommen.[104]

Zur gezielten Förderung von privatwirtschaftlichen Investitionen erscheinen die hier diskutierten Sonderabschreibungen aus mehreren Gründen als besonders geeignet. Die Förderung ist besonders zielgenau, da nur Unternehmen profitieren, die auch geförderte Investitionen tätigen. Angesichts der nach wie vor sehr günstigen Refinanzierungskonditionen des öffentlichen Sektors ist die Förderung zudem deutlich kostengünstiger als eine dauerhafte Senkung der Unternehmenssteuerbelastung. Im Hinblick auf das angestrebte Ziel (Förderung von Investitionen in langlebige Wirtschaftsgüter) sind Sonderabschreibungen somit wesentlich effektiver als eine vom Fördervolumen vergleichbare Senkung der Unternehmenssteuersätze. Eine Förderung sollte zeitlich befristet erfolgen um a) den Effekt auf das Investitionsvolumen zu maximieren und b) dauerhafte Verzerrungen der Investitionstätigkeit und Ineffizienzen zu vermeiden, die zu einer Senkung der Qualität von Investitionen führen können.[105]

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[101] Vgl. IMK, 2019, S. 3.

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Chapter 5

How to Account for Tax Planning and Its Uncertainty in Firm Valuation?

How to Account for Tax Planning and Its Uncertainty in Firm Valuation?

Abstract

I compare two approaches from the recent literature on how to account for tax planning and its uncertainty in a valuation framework (the separate view of Drake, Lusch, and Stekelberg 2019 vs. the composite view of Jacob and Schütt 2020), emphasizing measurement issues of tax planning and firm heterogeneity. Replication analyses and extensive robustness tests suggest that only considering tax planning and it's uncertainty jointly and connecting them to firm value via income leads to consistent results, implying that higher uncertainty-adjusted tax planning amplyfies the positive association between pre-tax income and firm value. However, the economic magnitude of this association depends on the measurement approach, ranging between 0.8% and 12.91%. Conversely, the separate view produces inconsistent results in all tests. These conclusions are not affected by incorporating recent losses (Dyreng et al. 2021) when an appropriate tax planning measure is chosen. While the results become insignificant when effective tax rates are used, applying the measure of Henry and Sansing (2018) mitigates this problem. Moreover, the positive value implication of uncertainty-adjusted tax planning is particularly pronounced for firms with low leverage whose debt tax shield and debt overhang are relatively small. The logic of jointly measuring tax planning and its uncertainty seems to be extendable to a variety of measures and to provide a more suitable measure of tax planning than traditional isolated effective tax rates in a valuation framework.

JEL classification codes: G32; H25; H26; M21; M41

Keywords: Tax Planning; Tax Uncertainty; Firm Value; Tax Planning Score

5.1. Introduction

This paper analyses the link between corporate tax planning (TP), tax uncertainty (TU), and firm value (FV) for the case of listed German firms by i) comparing two recent approaches to account for TU in a valuation framework (Drake, Lusch, and Stekelberg 2019 vs. Jacob and Schütt 2020), ii) assessing the dependence of results on the measurement of TP, especially in the presence of losses (Henry and Sansing 2018; Dyreng et al. 2021), and iii) examining firm heterogeneity in the responsiveness of firm valuation to TP. From a traditional net present value perspective, TP leads to lower tax burdens and higher after-tax cash flows for firms, increasing their value. However, negative effects such as reputational costs (Gallemore, Maydew, and Thornock 2014) or higher tax-induced uncertainty (Guenther, Wilson, and Wu 2019) can mitigate these positive effects. Recently, two approaches to incorporate TU in a valuation framework have been developed: the separate view by Drake, Lusch, and Stekelberg (2019) which treats TP and TU as distinct constructs and connects them directly to FV, and the composite view by Jacob and Schütt (2020) which combines TP and TU into one measure and links them indirectly to FV through pre-tax income. Drake, Lusch, and Stekelberg (2019) find that TP (TU) is positively (negatively) associated with FV, while TU dampens the positive relationship between TP and FV. Jacob and Schütt (2020) provide evidence that the positive association between pre-tax income and FV is enhanced by higher values of uncertainty-weighted TP (measured by the Tax Planning Score, TPS), and provide a rationale for why the separate view might suffer from model misspecification: TP and TU should be considered together, because investors need to build expectations for the future based on past information. In doing so, they care about the information content (i.e., the uncertainty) of the effective tax burden, not just its amount.

Nevertheless, Drake, Lusch, and Stekelberg (2019) have received more attention by subsequent studies: They are cited significantly more often than Jacob and Schütt (2020),[1] and the notion that TP and TU should be considered together is not yet widespread in the literature – despite the fact that the earliest version of Jacob and Schütt (2020) has been published as a working paper since 2013 (Jacob and Schütt 2013). In addition, the TP literature on valuation since 2020 only considers TP and TU separately (e.g., Irawan and Turwanto 2020; Firmansyah and Widodo 2021; Firmansyah, Febrian, and Falbo 2022; Seifzadeh 2022), while the role of TU is often completely neglected (e.g., Chukwudi, Okonkwo, and Asika 2020; Khuong et al. 2020; Rudyanto and Pirzada 2021; Arora and Gill 2022; Inger and Stekelberg 2022). Similarly, recent studies examining the association between TP, TU, and various economic outcomes do not include the TPS as a measure

[1] Drake, Lusch, and Stekelberg (2019) (Jacob and Schütt 2020) is cited 153 times (73 times), as documented by Google Scholar, 3rd of April 2023, while the majority of studies on TP and FV only cite Drake, Lusch, and Stekelberg (2019) (see Section 5.2).

(Dhawan, Ma, and Kim 2020; He, Ren, and Taffler 2020; Osswald 2020; Dos Santos and Rezende 2020; Abernathy et al. 2021; Gkikopoulos, Lee, and Stathopoulos 2021; Adams et al. 2022; Purwaka et al. 2022).[2] One reason for not using the Jacob and Schütt (2020) model and measure could be that the composite view does not come without caveats. A disadvantage of measuring TP and TU together is that it is difficult to interpret composite values in an intuitive or plausible way. Likewise, the relative importance of TP and TU in valuation might be diluted if they are combined. After all, the model in Jacob and Schütt (2020) is based on debatable theoretical assumptions (e.g., simplifying abstractions in the residual income model and the way investors form expectations) and also might be more subject to measurement error than Drake, Lusch, and Stekelberg (2019) as it is based on a very specific specification. Therefore, the separate view might still more accurately capture the differential impacts of the level of TP and TU on FV and might be easier to interpret.

Hence, a comparison of both approaches in terms of their robustness and suitability in a valuation framework seems useful, since a comprehensive investigation of both views has not yet been conducted. In addition, this paper extends the analysis by examining (i) the dependence of results on the choice how to measure TP and (ii) the heterogeneity of firms in their responses. The accounting literature to date has relied on various TP measures (e.g., GAAP or cash ETRs, book-tax-differences) and empirical specifications (e.g, control variables), so it is not clear how the results depend on (at times arbitrary) measurement choices (De Simone et al. 2020). In particular, Henry and Sansing (2018) point out that TP studies may suffer from data truncation bias due to the exclusion of loss-making firms, which are often omitted because it is difficult to interpret traditional TP measures when losses are present. They develop a new TP measure (Delta MVA) that is interpretable in loss cases. However, similar to the TPS, this measure has not been widely applied in the recent literature. Related to this, Dyreng et al. (2021) demonstrate that low values of traditional TP measures are likely to be misinterpreted as incremental TP, when in fact they are an accidental byproduct of recent losses. It could be that the expected – and in Jacob and Schütt (2020) documented – positive association between (uncertainty-adjusted) TP and FV changes when these aspects are taken into account. Moreover, not much attention has been paid to the question for which type of firms the relationship between TP and FV is particularly pronounced. Since Jacob and Schütt (2020) rely on the residual income model and therefore focus on equity valuation, they abstract from the impacts of debt. However, prior literature has shown that firms' leverage can have an impact on their equity value due to debt overhang and default risk (Myers 1977; Cai and

[2] Jacob and Schütt (2020) note that "in any setting where expectations about future tax rates are important, adequately incorporating tax uncertainty is crucial for assessing the role of tax avoidance." (p. 411) There is no apparent reason indicating that the conclusions about the need to include TU in TP analyses are exclusive to the topic of valuation.

Zhang 2011). More directly related to tax issues, the value of the debt tax shield declines with lower effective tax rates (and thus higher TP) due to the deductibility of interest expense from the tax base. The level of firms' debt holdings might therefore matter for investors' valuation of TP. The role of available resources of firms is also investigated, as prior literature has shown that costs of TP can influence the intensity of its impact on economic outcomes (e.g., Eichfelder and Vaillancourt 2014; Hundsdoerfer and Jacob 2019).

The results from the replication and comparison of both views indicate that only the composite view with the TPS leads to robust and consistent results. Nonetheless, the economic magnitude of the positive association between uncertainty-weighted TP and FV depends on the used TP measure and time horizon: on average, a one standard deviation increase in the TPS leads to a 0.8–12.91% increase in the positive association between pre-tax income and the price-to-book ratio. Conversely, the separate view yields inconsistent results, most of which are not statistically significant and vary widely across different TP measures and control settings.

Regarding the role of losses in measuring TP, the results of the composite view are robust to i) applying the measure of Henry and Sansing (2018) to the TPS logic in a loss sample, and ii) the Dyreng et al. (2021) approach to control for incidental TP when an appropriate TP measure is used. While the results with ETRs become essentially zero similar to Dyreng et al. (2021) – even as a basis of the TPS – using the Henry and Sansing (2018) measure again leads to consistent results.

Lastly, I find robust evidence that the positive relationship between the TPS, pre-tax income, and FV is especially pronounced in low leveraged firms, confirming the intuition that TP (apart from debt financing) is more beneficial when the debt tax shield and issue of debt overhang are relatively small. There is also some evidence that firms with less resources receive stronger positive value implications of uncertainty-weighted TP, which could be explained by benefits from TP being valued relatively stronger in firms for which (cash) benefits from TP are larger. These results, however, are not robust in all specifications.

This paper contributes to the literature in several ways. First, by replicating the separate and composite view for the case of listed German firms, the role of TP and TU in valuation is assessed in a new capital market environment. At the same time, the comparison of both views can guide future studies on which approach should be used – especially since the composite view has not received much attention yet. Second, the analyses contribute to the literature on the importance of methodological choices in empirical TP studies (De Simone et al. 2020) by showing how estimates vary with different TP measures that are frequently applied. Arbitrarily selecting only one or a few of them when reporting results might bear the risk of over- or understating the economic magnitude of associations. Third, the analyses also contribute to the discussion how losses affect TP measures and outcomes (Henry and Sansing 2018), and to the question recently raised in Dyreng et al. (2021) whether previous firm losses lead to misinterpretation of TP measures. While this indeed seems to be an issue in a valuation framework as well when ETRs are used, other measures may be better suited to account for recent losses – in particular, the measure of Henry and Sansing (2018). Lastly, this paper also contributes to the literature documenting heterogeneous responses of corporate economic outcomes to taxes and TP (e.g., Büttner, Overesch, and Wamser 2011; Zwick and Mahon 2017; Jacob 2022).

The remainder of the paper is structured as follows. Section 5.2 briefly discusses and summarizes related literature. Section 5.3 recapitulates the intuition and theoretical background of the separate and composite view. The empirical approach and data are described in Section 5.4, while the results are presented in Section 5.5. Finally, Section 5.6 concludes.

5.2. Related Literature

The literature on the relationship between TP and FV generally consists of several strands that either directly address the issue or have indirect implications for valuation.[3] Taxes can affect FV through at least three channels: (i) taxes directly affect firms' after-tax cash flows and earnings; (ii) taxes affect the after-tax cost of capital (Sikes and Verrecchia 2020); and (iii) taxes determine the degree of risk sharing with the government (Desai and Dharmapala 2009). TP activities affect the first channel positively, as lower tax payments lead to higher after-tax cash flows and profits. However, TP can also increase the uncertainty of future after-tax outcomes, i.e., they become more volatile. If investors prefer a smooth development of earnings (Neuman 2014), their required return would increase. Thus, closely related to the objectives of this paper are studies on the association between TP and the cost of equity (CoE). A similar reasoning applies to the third channel: The lower the effective tax burden, the higher the share of risk borne by the firm rather than by the government. How investors ultimately value TP depends on which effect dominates. The literature directly addressing this issue is relatively sparse (Hanlon and Heitzman 2010). Studies published after the contributions this paper focuses on (Drake, Lusch, and Stekelberg 2019 and Jacob and Schütt 2020) primarily investigate TP and FV in emerging markets (e.g., Irawan and Turwanto 2020; Firmansyah and Widodo 2021; Firmansyah, Febrian, and Falbo 2022; Seifzadeh 2022). In the following, the strands are

[3]For example, studies examining the association between TP and the cost of equity (CoE) (Goh et al. 2016; Cook, Moser, and Omer 2017) or stock returns (Heitzman and Ogneva 2019) do not directly address the relationship with FV, but CoE are relevant in valuation formulas and are often measured by market returns.

summarized, starting with the link between TP and the CoE.

Tax planning and the cost of equity A considerable part of the empirical literature argues that corporate TP induces non-diversifiable risk that leads to higher CoE. Most of these studies assume that the risk associated with TP arises from uncertainty about future tax policy (Brown et al. 2014) and affects economic risk through investment returns (Guenther, Matsunaga, and Williams 2017). Brown et al. (2014) show that investors perceive TP benefits as risky during periods of high uncertainty in the tax policy environment. This, in turn, increases the investors' risk assessment of investments. Heitzman and Ogneva (2019) use U.S. data and distinguish between periods under Republican and Democratic administrations. Their findings of a positive association between TP and stock returns are almost entirely explained by the "tax-friendly" Republican terms. Sikes and Verrecchia (2020) show that aggregate TP at the industry level is associated with higher CoE, as the uncertainty of a firm's future cash flows increases with the uncertainty of TP activities in the firm's industry. These results suggest negative value implications of TP as investors demand a higher future return on investment, which depresses current value.

Conversely, studies examining the direct relationship between firm-level TP and CoE (e.g., Hutchens and Rego 2013 and Goh et al. 2016) suggest that TP can also have a positive effect on CoE, implying that they decrease the higher a firm's TP level is. However, Hutchens and Rego (2013) argue that this depends on the type of TP and show that the uncertainty caused by some TP strategies can instead lead to higher CoE. The results in Hanlon and Slemrod (2009) have already supported this by showing a negative correlation between stock prices and the aggressiveness of TP. Overall, the literature on the association between TP and CoE provides mixed results and relies on models that only focus on TP or uncertainty separately.

Tax planning, risk and firm value Desai and Dharmapala (2009) place emphasis on agency theory, which recognizes the difference between ownership and control of firms, and find that the association between TP and FV depends strongly on the quality of corporate governance. Also in Kim, Li, and Zhang (2011), a positive association between TP and the risk of an abnormally large decline in stock prices is attributed to the agency principle. These earlier studies (as well as those on CoE) have not considered the uncertainty of TP as a unique concept. Vello and Martinez (2012) find that more efficient TP strategies significantly reduce market risk, depending on good corporate governance. In contrast, Assidi (2015) conducts a case study for 40 listed French companies and finds a positive relationship between ETRs, or their volatility, and firm risk. Hutchens and Rego (2015) relate various measures of tax risk to firm risk and find that only the volatility of cash ETRs and book-tax differences are significantly associated with firm risk. Other measures show either a negative association or none at all. Brooks et al. (2016) examine the relationship between tax payments and financial performance in the United Kingdom, finding that firms' ETRs do not affect stock returns but are negatively associated with market risk. Nesbitt, Outslay, and Persson (2017) show for a sample of firms affected by the Luxembourg Leaks that investors responded positively to the exposure, which could be explained by a reduction in uncertainty. Finally, Guenther, Matsunaga, and Williams (2017) conceptually distinguish between TU, TP and tax aggressiveness and document a positive association between TU and firm risk. However, they do not find a direct association between TP itself and firm risk.

Considering that TU might affect value-relevant outcomes, two recent studies offer approaches to empirically account for both TP and TU. Drake, Lusch, and Stekelberg (2019) treat the degree of TP and its uncertainty as distinct constructs, while Jacob and Schütt (2020) combine them into a composite measure (Tax Planning Score, TPS). Unlike previous studies, Jacob and Schütt (2020) do not attempt to find a direct relationship between TP, TU, and FV, but suggest that their relation is determined by pre-tax income channels, while Drake, Lusch, and Stekelberg (2019) interact measures of TP (ETRs) with measures of TU (volatility of ETRs) and link them directly to FV. The value of the firm decreases with TU and increases with the degree of TP, while this positive association is attenuated by TU. In Jacob and Schütt (2020), firms with higher TPS values, which increases with the degree of TP and decreases with TU, experience a stronger positive relationship between pre-tax income and FV.[4]

Jacob and Schütt (2020) provide empirical tests to assess whether past ETRs (ETR volatilities) are appropriate predictors of future ETRs (ETR volatilities) and emphasize the need to weight available tax information according to its information content for investors. However, studies published after these two papers do not seem to have acknowledged Jacob and Schütt (2020)'s valuation model – in particular, the notion that TP and TU levels should be considered together. Irawan and Turwanto (2020), Firmansyah and Widodo (2021) and Firmansyah, Febrian, and Falbo (2022) apply Drake, Lusch, and Stekelberg (2019)'s approach to Indonesian firms and find mixed results: Irawan and Turwanto (2020) find that both TP and TU are positively associated with FV, while TU moderates this relationship. Firmansyah and Widodo (2021) do not interact both concepts, finding that TP (TU) is positively (negatively) associated with FV. When TP and TU are interacted in Firmansyah, Febrian, and Falbo (2022), the results are similar to Drake, Lusch, and Stekelberg (2019). In addition, Khuong et al. (2020) examine TP and firm performance in Vietnam and find mixed results, and Seifzadeh (2022) focus on the role of managerial ability in the relation between TP and FV in Iran, finding a negative association between TP and FV that is less strong in firms with high ability managers. In

[4] Drake, Lusch, and Stekelberg (2019) note that they obtain similar results to Jacob and Schütt (2020) when applying the composite approach, but Jacob and Schütt (2020) cannot replicate their results and provide a rationale for why the separate model is likely to be misspecified in simulations.

these two studies, TU as a concept is completely neglected. The same is true for Chukwudi, Okonkwo, and Asika (2020), which examines public firms in Nigeria, Rudyanto and Pirzada (2021) claiming that sustainability reporting could moderate the link between TP and FV, Arora and Gill (2022) showing that TP is negatively associated with FV, and Inger and Stekelberg (2022), who provide evidence that only socially responsible TP is positively valued by investors. In all of these studies, Jacob and Schütt (2020) is not cited (except for Irawan and Turwanto 2020), while Drake, Lusch, and Stekelberg (2019) is, and the composite approach is therefore not acknowledged.

To sum up, although the earliest version of Jacob and Schütt (2020) had been in circulation since 2013 (Jacob and Schütt 2013), their valuation model is not widely used in the literature. The same is true for the joint measure TPS, which is not included in studies on the relation between TP and FV or the connection of TP/TU to other economic outcomes (Dhawan, Ma, and Kim 2020; He, Ren, and Taffler 2020; Osswald 2020; Dos Santos and Rezende 2020; Abernathy et al. 2021; Gkikopoulos, Lee, and Stathopoulos 2021; Adams et al. 2022; Purwaka et al. 2022). To the best of my knowledge, the study by Brooks et al. (2016) is the only one that explicitly refers to the TPS as a measure, but does not conduct analyses with it. Thus, in the following, different empirical specifications and TP measures are applied to the separate and composite views to evaluate the notion of Jacob and Schütt (2020) with a focus on empirics, before turning to potential extensions.

5.3. Theoretical Background and Intuition

5.3.1. Separate View

Drake, Lusch, and Stekelberg (2019) derive their hypotheses from prior empirical work, where TP is allegedly associated with higher FV on average. However, as described above, the literature does not find this positive relationship for all forms of TP. The separate view relies on the CAPM logic that non-diversifiable risk leads to higher risk premia and on the model extension of Sikes and Verrecchia (2020) from Lambert, Leuz, and Verrecchia (2007). In this framework, the uncertainty of firms' after-tax cash flows increases with the uncertainty of the TP strategies of the entire market or industry in which the firms operate. Drake, Lusch, and Stekelberg (2019) conclude that higher TU should lead to lower FV and lower positive value implications of TP.

This logic is subject to some caveats. First, the Sikes and Verrecchia (2020) model develops a framework in which *aggregate* TP of *industries* is the main variable of interest, not individual firm-level tax outcomes. Second, while there is a clear trade-off between risk and return in the CAPM model, the relationship between the degree of TP and it's uncertainty is not as clear. The results in Guenther, Matsunaga, and Williams (2017;

2019) suggest that lower ETRs are actually more persistent on average than high rates, implying that high TP can be achieved by relatively riskless strategies that do not induce much uncertainty. Lastly, separating TP and TU theoretically implies that investors perceive both concepts as value-relevant independently of each other.

While Drake, Lusch, and Stekelberg (2019) do not develop a clear theoretical argument as to why the separate consideration of TP and TU is appropriate, the intuitions in favor and against this can be reduced to the following. TP can be broadly defined as a spectrum of activities that reduce tax liability (Hanlon and Heitzman 2010). This can include high-risk and even gray-area strategies, as well as actions that are persistent and do not carry the risk of penalties, tax policy uncertainty, and reputational costs. On the one hand, separating TP and TU may not recognize this heterogeneity of strategies. On the other hand, it is also possible that TP and TU are related to FV independently. As Guenther, Matsunaga, and Williams (2017) show, aside from the persistence of ETRs, tax risk is positively associated with firm risk, while the level of TP is not. This indicates that it may be important to separate the two concepts to determine which is more important, or whether only one of the two affects FV – similar to firm risk.

5.3.2. Composite View

The composite view is based on the residual income model (Feltham and Ohlson 1995) and provides a rationale for the need to consider TP and TU together. According to the model of Jacob and Schütt (2020), the current market value of firm i at time t can be written as follows:

$$M_{i,t} = B_{i,t} + \mathbb{E}_{i,t}\left[\sum_{t=1}^{\infty} \frac{RI_{i,t}}{(1+r)^t}\right] \tag{5.1}$$

where M is the market value, B is the book value, RI is the residual income and r is the CoE. The after-tax residual income in t is: $RI_{i,t} = \delta_{i,t} \cdot (I_{i,t}^{pretax} - r^{pretax} \cdot B_{i,t-1})$, where I is the after-tax income and δ is a tax multiplier. Future δ outcomes are assumed to fluctuate around their mean: $\delta_{i,t+1} = \mu_\delta + \epsilon_{i,t+1}$. Eq. 5.1 can then be expressed as:

$$M_{i,t} = B_{i,t} + \mathbb{E}_{i,t}\left[\mu_\delta\right] \cdot D_{i,t} \cdot RI_{i,t}^{pretax} \tag{5.2}$$

where D is a discount factor that takes into account the future evolution of income. The key parameter of interest, μ_δ, is uncertain. Jacob and Schütt (2020) assume that investors rely on information about the past tax rate volatility to determine the expected value of future tax rates today. Average future tax rates are uncertain along two dimensions: both statutory tax rates, s, and firm ETRs, τ, are uncertain, while they are both assumed to be normally distributed. Dividing both sides of Eq. 5.2 by the book value, and writing out the tax term $\mathbb{E}_{i,t}\left[\mu_\delta\right]$ gives:

$$\frac{M_{i,t}}{B_{i,t}} = 1 + \frac{\frac{1}{\sigma_s^2}\delta_s + \frac{n}{\sigma_{i,\tau}^2}\bar{\delta}_{i,\tau}}{\frac{1}{\sigma_s^2} + \frac{n}{\sigma_{i,\tau}^2}} \cdot D_{i,t} \cdot \frac{RI_{i,t}^{pretax}}{B_{i,t}} \tag{5.3}$$

which is the final valuation formula in Jacob and Schütt (2020). The factor right before $D_{i,t}$ formally expresses the intuition that the more volatile tax rates are expected to be (σ_s and σ_τ), the lower the information content (the higher the uncertainty). Thus, the tax parameter is an uncertainty-weighted tax rate, implying that investors rely on past information to form expectations. Jacob and Schütt (2020) develop the Tax Planning Score (TPS) to estimate the tax parameter, which relates the level of TP to the corresponding uncertainty (see Section 5.4). The main difference with Drake, Lusch, and Stekelberg (2019) with respect to TP can be found here, as TP and TU are not assumed to be independent. Moreover, the tax term interacts with pre-tax income (RI^{pretax}/B).

Taken together, the composite view differs from Drake, Lusch, and Stekelberg (2019) in two key ways: i) the way TP and TU are linked to FV (indirectly through pre-tax income rather than directly), and ii) the way TP and TU are measured (jointly rather than separately). Although the theoretical considerations of Jacob and Schütt (2020) imply that treating TP and TU separately is likely to be misspecified, a disadvantage of measuring TP and TU together is that it becomes impossible to evaluate their incremental effects. Weighting TP by its uncertainty may also result in composite values that are difficult to compare across firms or interpret in a plausible way. For example, a firm with an ETR of 10% and a volatility of 90% would have the same uncertainty-weighted TP value under the TPS logic as a firm with an ETR of 90% and a volatility of 10%. Therefore, considering them separately might still produce results that are easier to interpret and also more accurately explore the potentially different effects of TP and TU on FV.

5.3.3. Extensions

Losses and Measurement

While especially Jacob and Schütt (2020) emphasize the need to measure TP carefully, neither approach explores the role of losses. Henry and Sansing (2018) have already shown that relying on samples with only positive income (as both studies do) can result in data truncation bias. In a recent working paper, Dyreng et al. (2021) argue and provide evidence that the results of TP studies may be inflated by measuring incidental TP due to prior loss years rather than incremental TP independent of loss carryforwards. They show that ETRs are systematically lower the more prior loss years there are. Without recognizing the role of losses, these small values would simply be interpreted as high TP.

Even when measures are calculated over multiple years, as in Drake, Lusch, and Stekelberg (2019) and Jacob and Schütt (2020), they could still be affected by loss years that precede the relevant time window. The empirical approach of Dyreng et al. (2021) to investigate this issue is to control for recent losses in the regressions. Since most previous studies used ETRs, their analyses and replications focus only on these (e.g., for Hasan et al. 2014). While the separate view is likely similarly affected (at least when ETRs are used), it is not clear whether the TPS measure is also biased by recent losses, since TP is weighted by it's volatility. To be affected similarly to isolated ETRs, TPS values would have to increase systematically with the number of recent loss years. Furthermore, Henry and Sansing (2018) have already developed a TP measure, Delta MVA (D_MVA), that is explicitly designed to capture TP when losses are present. Thus, it may be that their measure is better suited to capture the role of recent losses. Further analysis after replicating the composite and separate views will therefore focus on these issues.

Firm Heterogeneity

An aspect that is unrelated to measurement issues and that has not yet been investigated in either approach is the question for which type of firms the proposed positive relationship between TP (adjusted for uncertainty) and FV is particularly pronounced. Naturally, since the residual income model expresses the market value of equity, the role of debt is not considered in the composite approach. Debt, however, can have an impact on the equity valuation of firms due to its influence on future investments (Myers 1977) and default risk. As Cai and Zhang (2011) show, changes in firms' leverage ratios are negatively associated with stock prices, especially for highly leveraged firms. In addition, standard valuation models that are based on a discounted cash flow (DCF) logic suggest that there is a debt tax shield due to the deductibility of debt interest from the firm's tax base, in the form (Kruschwitz and Löffler 2006):

$$Tax_t^u - Tax_t^l = \tau \cdot i \cdot Debt_{t-1}, \tag{5.4}$$

where Tax_t^u and Tax_t^l represent the tax payments of an unleveraged and a leveraged firm, respectively, τ is the ETR, and i is the (debt) interest rate. The association between TP and FV is likely to be less pronounced for highly leveraged firms, because i) debt overhang (Myers 1977) and default risk (Cai and Zhang 2011) become more of an issue, and ii) debt and TP can be viewed as substitutes to some extent: the higher the leverage ($Debt$), the higher the debt tax shield. Accordingly, more TP (a lower τ) might become less valuable if there is a high debt tax shield: the larger τ, the greater the benefit of debt-induced deductions.

Moreover, previous studies have shown that the costs of tax compliance and TP activities are quasi-fixed (e.g., Eichfelder and Vaillancourt 2014; Hundsdoerfer and Jacob 2019). Firms that have more resources (e.g., large and high cash flow firms) may benefit more from (uncertainty-adjusted) TP than their counterparts, since their marginal costs of engaging in TP are lower. On the other hand, firms with less resources are more capital constrained, so they could gain larger relative cash flow benefits from TP than their peers. I refrain from making a clear prediction about which type of these firms responds more strongly and leave this question open for empirical investigation in the additional analyses.

5.4. Method and Data

5.4.1. Measures of Tax Planning and Tax Uncertainty

The analyses follow the broad definition of TP by Hanlon and Heitzman (2010), where corporate TP comprises all activities that reduce the firm's tax liability. This has the merit of including both high-risk and riskless planning strategies. The most common measures used by prior literature are effective tax rates (ETRs), which relate income tax expense or cash taxes paid to the tax base. While cash ETRs (*CETR*s) incorporate tax deferral strategies, GAAP ETRs (*GETR*s) exclude them by definition. Since this study relies on data for German corporations from Datastream (see Section 5.4.3), where cash taxes paid structurally have many missings, I use the GETR as the main measure for the baseline analyses. However, GETRs are more susceptible to be biased by earnings management, since both the numerator and denominator consist of balance sheet items. Therefore, in line with Drake, Lusch, and Stekelberg (2019) and Jacob and Schütt (2020), CETRs are also used.[5] To further assess robustness, I also use book tax differences (BTD), their permanent component ($PBTD$), and the measure of Henry and Sansing (2018) (D_MVA) which is designed to account for loss years in the sample and will therefore play a larger role in the additional analyses on the importance of recent losses in Section 5.5.2.

Dyreng, Hanlon, and Maydew (2008) suggest to calculate long-run measures over 10 years to reduce potential measurement errors due to year-to-year fluctuations. However, this procedure results in a significant loss of variation and observations. I therefore calculate the GETR (and all other measures) over a rolling 5 year window as follows:

$$GETR_{i,t} = \frac{\sum_{z=t-4}^{t} IncomeTaxes_{i,z}}{\sum_{z=t-4}^{t} PretaxIncome_{i,z}} \tag{5.5}$$

Turning to TU, empirical proxies differ in their ability to capture different types of tax aggressiveness (Blouin 2014). While provisions for unrecognized tax benefits (UTB)

[5] A potential limitation of the data base with respect to the TP measures – particularly ETRs – is that variation in ETRs may be partly driven by foreign tax rates, as the firms in the sample operate internationally. Due to data limitations, the (weighted) tax rates of the group to which the firm belongs — or is a parent of — are not available.

are commonly used in U.S. samples (e.g., Lisowsky, Robinson, and Schmidt 2013; Ciconte et al. 2016), German accounting rules do not require firms to disclose these items. In the replication analyses, I rely on ETR volatilities as a measure of TU, since they capture the dispersion of potential tax outcomes. In a valuation framework, investors need to rely on past information that is available to them in a timely manner, while Guenther, Matsunaga, and Williams (2017) provide evidence that the volatility of ETRs is an appropriate measure of tax risk. Consistent with the definition of TP in Eq. 5.5, TU is therefore calculated as the standard deviation of the GETR over a rolling 5 year window:

$$VolGETR_{i,t} = \sqrt{\sum_{z=t-4}^{t} \left(GETR_{i,z} - Mean(GETR_i)\right)^2} \tag{5.6}$$

Finally, TP and TU are combined to calculate the Tax Planning Score (TPS), which relates the level of TP to the associated uncertainty:

$$TPS_{i,t} = \frac{1 - GETR_{i,t}}{VolGETR_{i,t}} \tag{5.7}$$

The TPS increases with TP (numerator) and decreases with TU (denominator), recognizing that firms can achieve certain levels of GETRs with different corresponding risk. Jacob and Schütt (2020) note that they are agnostic about the basis of their composite measure. Their logic only postulates that some measure of TP should be in the numerator, while a measure of TU should be in the denominator according to Eq. 5.3. Therefore, I calculate all the measures described with the CETR, BTD, PBTD, and D_MVA as alternative proxies. Table 5.1 shows all tax variables, along with their description.

5.4.2. Empirical Strategy

The first objective of this paper is to evaluate the separate and composite view in the same capital environment. For this exercise, I select the control variables as close as possible to the two original studies.[6] The two views are replicated by applying the following OLS regressions:

$$\begin{aligned} PTB_{i,t} = {} & \alpha_0 + \alpha_1 TP_{i,t} + \alpha_2 TU_{i,t} + \alpha_3 TP_{i,t} \cdot TU_{i,t} + \alpha_4 PI_{i,t} + \alpha_5 VolPI_{i,t} \\ & + \alpha_6 PI_{i,t} \cdot VolPI_{i,t} + \alpha_6 SalesGrowth_{i,t} + \alpha_7 X_{i,t} \\ & + \theta_i + \gamma_t + \epsilon_{i,t} \end{aligned} \tag{5.8}$$

[6] For example, the CoE are only part of the composite approach in the replication analyses. When changing the control settings in Section 5.5.1.2, the exact same specifications are applied to both views.

for the Drake, Lusch, and Stekelberg (2019) model. Eq. 5.3 can be written as a reduced-form OLS regression equation of the composite view as:

$$\begin{aligned} PTB_{i,t} = \beta_0 + \beta_1 PI_{i,t} + \beta_2 TPS_{i,t} + \beta_3 TPS_{i,t} \cdot PI_{i,t} + \beta_4 SalesGrowth_{i,t} \\ + \beta_5 SalesGrowth_{i,t} \cdot TPS_{i,t} + \beta_6 CoE_{i,t} + \beta_7 X_{i,t} \cdot Y_{i,t} \\ + \theta_i + \gamma_t + \epsilon_{i,t}, \end{aligned} \tag{5.9}$$

where $PTB_{i,t}$ is the price-to-book ratio of firm i in year t, TP is a measure of the degree of tax planning, TU measures tax uncertainty, TPS is the Tax Planning Score, CoE is the cost of equity (approximated by the stock return plus the risk-free interest rate), PI and $VolPI$ are the pre-tax income (scaled by the book value of common equity in line with Jacob and Schütt 2020) and its volatility, $SalesGrowth$ is the growth of sales over 5 years, X is a vector of additional controls (including cash flow volatility, stock price volatility, leverage, and depreciation expense), and θ_i and γ_t are firm and year fixed effects, respectively.[7] In all analyses, the tax planning measures used are standardized such that higher values of the variable TP always imply a higher degree of tax planning. All control variables are winsorized at the first and 99th percentiles. For a description of the main variables, see Table 5.1.

According to the intuition behind the separate view, α_1 in Eq. 5.8 is expected to be positive, α_2 negative, and α_3 also negative. The coefficient of interest in Eq. 5.9, β_3, is expected to be positive, since a higher TPS should amplify the positive association between pre-tax income and FV ($\beta_1 > 0$). To assess the robustness of both approaches, I apply different measures of TP and TU to equations 5.8 and 5.9, as well as different control settings that are oriented on specifications from prior literature. The dispersion of coefficient estimates across specifications is compared to assess whether the results depend on the measurement of firm characteristics that are commonly controlled for and operationalized in different ways (e.g., firm size, debt, operational risk).[8]

In the final step, I extend the results of Drake, Lusch, and Stekelberg (2019) and Jacob and Schütt (2020) by examining i) the role of recent losses on TP outcomes, and ii) firm heterogeneity in responses. For the loss analysis, I conduct the baseline analysis with a sample that includes loss years and add two indicator variables, as proposed by Dyreng et al. (2021), ($Loss5$ and $Loss5\%$) to control for incidental TP.

[7] Jacob and Schütt (2020) use industry fixed effects in their main analyses, while I use firm fixed effects throughout to control for unobserved firm characteristics. Hence, Eq. 5.9 exploits variation in the average TPS (and the other variables) within firms over time, while controlling for industry fixed also considers cross-sectional variation between firms.

[8] If the objective of previous studies differs slightly from the direct investigation of FV implications of TP, the control settings are adjusted. For example, controlling for the book-to-market ratio (Cook, Moser, and Omer 2017; Sikes and Verrecchia 2020) when the price-to-book ratio is the dependent variable clearly leads to biased estimates, so this variable is excluded from the respective settings.

Table 5.1: Variable Definitions

Firm Value & PI	Description	Formula
PTB	Price-to-Book ratio	$PTB = SharePrice/BookValuePerShare$
PI	Pretax Income scaled by Equity	$PI = PretaxIncome/CommonEquity$
Tax Planning Variables		
$GETR$	GAAP Effective Tax Rate	$GETR = IncomeTaxes/PretaxIncome$
$CETR$	Cash Effective Tax Rate	$CETR = CashflowTaxation/PretaxIncome$
BTD	Total Book-Tax-Differences	$BTD = (PretaxIncome - IncomeTaxes/STR)/TotalAssets$
$PBTD$	Permanent Book-Tax-Differences	$PBTD = BTD - (DeferredTaxes/STR)/TotalAssets$
D_MVA	Delta MVA (Henry and Sansing 2018)	$D\_MVA = (IncomeTaxes - PretaxIncome * STR)/MarketValueOfAssets$
TP	Tax Planning	$GETR$, $CETR$, BTD, $PBTD$, or D_MVA, standardized to always increase in tax planning
TU	Tax Uncertainty	Volatility of TP
TPS	Tax Planning Score	$TPS = (1 - TP)/TU$ for $GETR$, $CETR$, and D_MVA; TP/TU for BTD and $PBTD$
Control Variables		
$VolPI$	Volatility of PI	$VolPI = \sqrt{\sum_{z=t-4}^{t} (PI_{i,z} - Mean(PI_i))^2}$
CoE	Cost of Equity	$CoE_{i,t} = (SharePrice_{i,t} - SharePrice_{i,t-1})/SharePrice_{i,t-1} + RiskFreeReturn_t$
$SalesGrowth$	Sales Growth over the 5 previous years	$SalesGrowth_{i,t} = (\sum_{z=t-4}^{t} Sales_{i,z}/Sales_{i,z-1}) - 1$
$VolCF$	Cashflow Volatility	$VolCF_{i,t} = \sqrt{\sum_{z=t-4}^{t} (CashFlow_{i,z} - Mean(Cashflow_i))^2}$
$VolP$	Price Volatility	$VolP_{i,t} = \sqrt{\sum_{z=t-4}^{t} (SharePrice_{i,z} - Mean(SharePrice_i))^2}$
$Leverage$	Total Debt to Equity Ratio	$Leverage = (ShortTermDebt + LongTermDebt)/CommonEquity$
$Depreciation$	Depreciation Expenses	$Depreciation_{i,t} = DepreciationExpenses_{i,t}/TotalAssets_{i,t-1}$
Additional Variables		
$Loss$	Loss-Indicator for each year	$Loss = 1$ if $PretaxIncome < 0$
$Loss5$	Loss-Indicator previous 5 years	$Loss5$=1 if $Loss = 1$ in at least one of the previous 5 years
$Loss5\%$	Percentage of loss years in the last 5 years	$Loss5\% = (\sum_{z=t-4}^{t} Loss_{i,z})/5$

Note: This table shows the detailed description and calculation of the main variables for the baseline analyses. i indexes the firm, while t stands for the time index. For brevity, the indices are left out for the tax planning variables – these are calculated over 5 (8, 10) year rolling windows in the replication analyses (see Section 5.4.1) – and only reported when important for the control variables. The variable definitions are as close as possible to Drake, Lusch, and Stekelberg, 2019 and Jacob and Schütt, 2020. Since the data source is Datastream from Thomson Reuters, some information is not available compared to the aforementioned Compustat studies.

While all previous TP and TU measures are used, a particular focus is on the TP measure of Henry and Sansing (2018), which is designed to capture TP in the presence of losses. For the heterogeneity analyses, I divide the sample into high and low value firms in terms of leverage, firm size, and cash flows. In addition, I interact the term $TPS \cdot PI$ in Eq. 5.9 with the respective heterogeneity variable. A description of all variables can be found in Table 5.1.

5.4.3. Data and Descriptive Statistics

The balance sheet and equity data of publicly listed German firms for the sample period 2008–2018 stem from Datastream by Thomson Reuters. Information on the yield of ten-year German government bonds is acquired from the German Central Bank (*Deutsche Bundesbank*) as a measure of the risk-free rate of return for calculating the CoE. Since long-run measures over 5 years are used in the main analysis, data on tax expense and pre-tax income must be available from 2004 onward. Unlike Compustat and U.S. Data in general, cash taxes paid is a variable that is relatively rare in the German data.[9] Therefore, I rely on GAAP ETRs in the main analysis and use cash ETRs in robustness analyses. When replicating the separate and composite view, the sample is restricted to firm-years with positive pre-tax income and income tax expense. Starting with 6,667 firm-year observations for all publicly listed German firms that are active in the last sample year and for which information on pre-tax income and income tax expense is available, 1,915 observations with negative pre-tax income and 348 (180) observations with negative income tax expense (cash taxes paid) are dropped.[10] Finally, 2,199 observations are dropped due to the long-run horizons of the TP measures and missing information on the other control variables, resulting in a final sample with 2,035 firm-year observations.

Table 5.2 presents the descriptive statistics of the TP measures and main variables. The ETRs are winsorized at 0 and 1, while all other variables are winsorized at the first and 99th percentiles. The average firm has a price-to-book ratio of 2.51 and pre-tax return to equity (PI) of 0.20. There appears to be a large variation in the sample regarding the TPS depending on how it is measured. The logic behind the composite measure implies only that a measure of TP should be in the numerator, while a measure of TU should be in the denominator – there is no particular indication of which exact measure to use. The smallest TPS means can be found when book tax differences are used for calculation. Interestingly, the mean of D_MVA is negative, indicating that the average firm in the

[9]The item in Datastream for cash taxes paid is "Cashflow Taxation", which has more than twice as many missings as "Income Taxes".

[10]When the Henry and Sansing (2018) measure is calculated, these restrictions are not required. However, in the replication and corresponding robustness analyses, I use this measure for the same sample as the GETR to ensure that the results are not driven by different firms in the samples. Additional analyses in Section 5.5.2 rely on the full sample including loss years.

Table 5.2: Descriptive Statistics

Variable	Obs.	Mean	SD	P25	Median	P75
Firm Value & PI						
PTB	2035	2.510	2.382	1.190	1.880	2.880
PI	2035	0.199	0.147	0.110	0.173	0.244
Effective Tax Rates						
GETR	2035	0.290	0.100	0.249	0.297	0.333
VolGETR	2035	0.106	0.181	0.027	0.054	0.108
TPS_GETR	2035	24.643	34.181	6.515	13.166	27.112
CETR	1116	0.294	0.115	0.238	0.291	0.346
VolCETR	1116	0.126	0.098	0.059	0.095	0.162
TPS_CETR	1116	11.230	14.522	4.251	7.610	12.585
Book-Tax-Differences						
BTD	2035	0.074	0.054	0.039	0.062	0.093
VolBTD	2035	0.018	0.020	0.007	0.012	0.022
TPS_BTD	2035	7.412	7.968	2.488	4.795	9.131
PBTD	742	0.047	0.041	0.018	0.039	0.070
VolPBTD	742	0.033	0.026	0.015	0.026	0.043
TPS_PBTD	742	2.421	2.939	0.603	1.414	3.168
Delta MVA (Henry and Sansing 2018)						
D_MVA	2035	-1.532	1.430	-1.713	-1.174	-0.844
VolMVA	2035	0.137	0.448	0.026	0.055	0.111
TPS_MVA	2035	33.128	31.804	11.673	22.335	41.879
Controls						
VolPI	2035	0.072	0.099	0.027	0.045	0.081
VolCF	2035	0.037	0.048	0.014	0.025	0.042
VolP	2035	0.152	0.423	0.024	0.056	0.120
CoE	2035	0.100	0.371	-0.106	0.059	0.261
SalesGrowth	2035	0.011	2.320	-0.254	-0.003	0.254
Leverage	2035	0.190	0.177	0.028	0.159	0.300
Depreciation	2035	0.040	0.031	0.023	0.034	0.051

Note: Descriptive statistics are reported for the tax planning variables and all baseline variables. All variables, except *PTB*, *TPS*, and *D_MVA* can be interpreted in percentage terms. Effective tax rates are winsorized at 0 and 1, while all other variables are winsorized at the first and 99th percentiles. Table 5.1 contains a detailed variable description along with their calculation.

sample is "tax favored" , in contrast to Henry and Sansing (2018). When loss firms are included, the mean of the measure becomes larger, but remains negative (see Table 5.7).

Figure 5.1 graphically displays the distribution of the price-to-book ratio across GETR deciles (Panel 5.1a), GETR volatility deciles (Panel 5.1b), and TPS deciles (Panel 5.1c). The price-to-book ratio increases in the bottom GETR deciles and shrinks in the upper deciles, while the highest firm values are found in the middle. Contrary to the intuition that high TP is associated with a high FV, the lowest GETRs tend to be associated with a relatively low FV. The relationship between TU and FV is much clearer, as the highest price-to-book ratios in Panel 5.1b are distributed in the lowest volatility deciles. This could be an explanation for the inconclusive GETR distribution: Low GETRs could be obtained by risky strategies that are negatively valued by investors, while the relationship between TU and TP could be non-linear (Guenther, Matsunaga, and Williams 2017; Jacob and Schütt 2020). When both measures are combined in Panel 5.1c, the FV generally increases with higher TPS values.[11] Figure 5.1 provides preliminary evidence that the composite view may be better suited to account for TU in a valuation framework.

5.5. Regression Results

5.5.1. Comparing the Separate and Composite Views

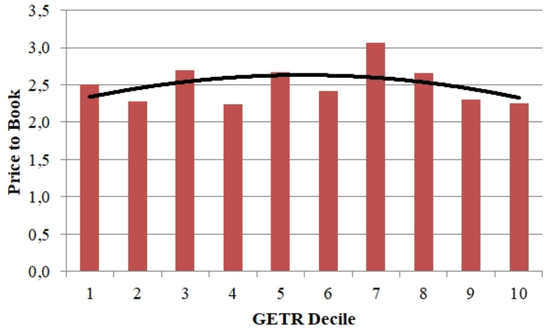
5.5.1.1. Replication Results

Starting with the replication of both views, the direction of the associations and the statistical significance of the coefficient estimates, as an indicator of the fit of the models, are compared.[12] Table 5.3 contains the results of estimating Eq. 5.8 to replicate the separate view. The first three columns report the coefficients without firm fixed effects, while the last three columns include all fixed effects. Control variables are added gradually rather than immediately to test the robustness of specifications. The coefficient estimates show that, as expected, there is a large positive association between pre-tax income and the price-to-book ratio. This association seems to be moderated by operating risk (negative and significant coefficients for the interaction terms of *PI* and its volatility and cash flow volatility).

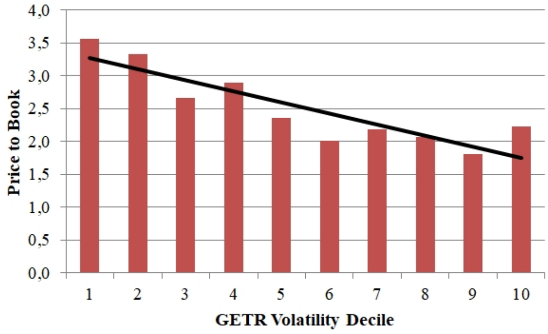
Regarding the TP variables of interest, there is neither systematic evidence that

[11]This is confirmed when the TPS is regressed on FV in isolation, see Table B.5.1 in Appendix B.

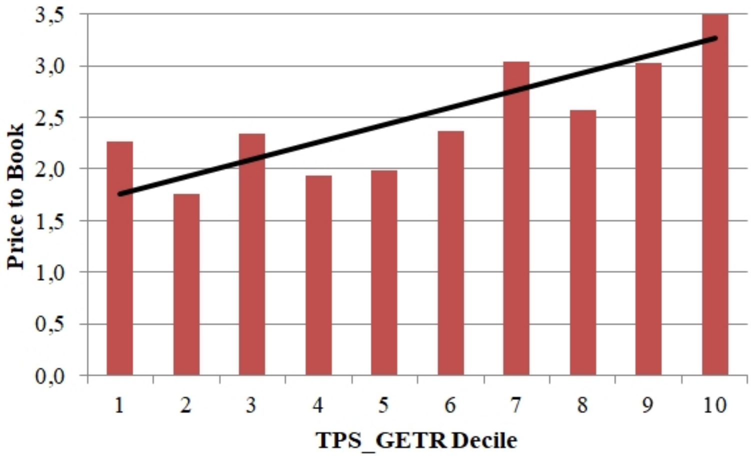
[12]Note that insignificant coefficients on the TP variables could not only stem from measurement error (separately vs. jointly), but also from investors not processing tax information efficiently. The separate view also implies that investors deem TP and TU as (equally) value relevant, while the TPS-model relies on the notion that the degree of TP needs to be adjusted by its information content (it's uncertainty).



a *GETR* and *PTB*



b *GETR* Volatility and *PTB*



c *TPS* and *PTB*

Figure 5.1: *PTB* and tax planning, tax uncertainty, and *TPS*

Notes: This figure shows the distribution of the price-to-book ratio (*PTB*) over TP deciles (GAAP effective tax rate, *GETR*), TU deciles (*GETR* volatility), and Tax Planning Score (*TPS*) deciles. The *GETR* is winsorized at 0 and 1. All other variables are winsorized at the first and 99th percentiles. In Panel (a), the highest firm values are found in the middle of the *GETR* distribution, while the lowest and highest deciles show similar values. Panel (b) shows a clear negative relationship between TU and FV. Higher *TPS* values tend to be associated with a higher price-to-book ratio (Panel c).

the degree of TP has positive value implications (since there are negative coefficients on *TP*), that TU has negative value implications (although most of the coefficients on *TU* are negative, they are not statistically significant), nor that TU moderates the association between TP and FV in the expected way: The coefficients on the interaction term between *TP* and *TU* are positive across most columns and mostly not statistically significant. Hence, the separate view does not yield significant results consistent with the intuition of Drake, Lusch, and Stekelberg (2019). Table 5.3 rather suggests that measures of operational volatility in the separate view are much more important than tax-related information (*VolPI*, *VolCF*, *VolP*).

As for the composite view, Table 5.4 shows the results of estimating Eq. 5.9. The columns refer to the same specifications as before. First, I again find positive and highly significant coefficients for *PI*. According to the estimate in column (6), a one standard deviation increase in *PI* (15%) is associated with an increase in the price-to-book ratio of 4.688 (186.77%), evaluated at sample mean values. As depicted in Table 5.6, the magnitude of this coefficient depends on the time horizon used to measure TP. Second,

Table 5.3: Separate View – Baseline

Variable	Base	+Risk	+Controls	Base	+Risk	+Controls
TP	-1.020	-0.128	0.069	-1.411*	-0.785	-0.783
	(0.622)	(0.660)	(0.660)	(0.739)	(0.789)	(0.787)
TU	0.034	-0.340	-0.340	0.471	-0.243	-0.299
	(0.542)	(0.577)	(0.577)	(0.563)	(0.597)	(0.597)
$TP\#TU$	1.157	0.099	-0.013	2.481*	1.420	1.291
	(1.337)	(1.296)	(1.295)	(1.405)	(1.364)	(1.362)
PI	6.154***	7.043***	7.236***	5.858***	6.683***	6.986***
	(0.252)	(0.444)	(0.447)	(0.258)	(0.494)	(0.501)
$VolPI$	0.484	-1.102	-1.001	1.126**	-0.432	-0.012
	(0.473)	(1.056)	(1.056)	(0.522)	(1.130)	(1.137)
$PI\#VolPI$		-3.659***	-3.935***		-2.765***	-3.252***
		(0.782)	(0.787)		(0.798)	(0.809)
$TP\#VolPI$		-8.300**	-8.229**		-5.022	-3.548
		(3.510)	(3.530)		(3.799)	(3.856)
$TU\#VolPI$		-0.315	-0.532		3.072*	3.198*
		(1.800)	(1.799)		(1.853)	(1.853)
$SalesGrowth$		-0.000	-0.000		-0.000	-0.001
		(0.010)	(0.010)		(0.010)	(0.010)
$VolCF$		4.055***	3.990***		3.610**	3.589**
		(1.240)	(1.237)		(1.446)	(1.444)
$PI\#VolCF$		-13.827***	-14.103***		-14.230***	-15.370***
		(3.544)	(3.530)		(4.871)	(4.875)
$VolP$		0.297	0.308		0.837***	0.831***
		(0.192)	(0.191)		(0.232)	(0.232)
$PI\#VolP$		5.278***	5.237***		2.894***	2.936***
		(0.770)	(0.769)		(0.819)	(0.817)
$Leverage$			-0.161			0.653*
			(0.300)			(0.365)
$Depreciation$			6.399***			6.586***
			(1.816)			(2.304)
Firm FE	No	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,035	2,035	2,035	2,035	2,035	2,035
R-squared	0.356	0.412	0.414	0.358	0.417	0.421

Note: This table reports the results from regression Eq. 5.8 to replicate the separate view by Drake, Lusch, and Stekelberg, 2019. The dependent variable is the price-to-book ratio. Columns (1)–(3) include year fixed effects; columns (4)–(6) include year and firm fixed effects. Controls are added in the columns in packages, indicated by the column heading. Standard errors are reported in parentheses. ***, ***, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

the coefficient for *TPS* is slightly negative. However, the model of Jacob and Schütt (2020) makes no prediction about the direct relationship between the TPS and FV.[13] Most importantly, the estimates for the interaction term between *TPS* and *PI* are positive and significant at the 1%-level, consistent with the theoretical prediction. The economic magnitude of the association is relatively small when using the GETR as a measure: In the most comprehensive model (6), a one standard deviation increase in *TPS* increases the coefficient of *PI* by 0.037, which is about 0.8% compared to the baseline coefficient of *PI*.[14] The association between *TPS*, *PI* and FV does not appear to be driven by operational volatility, as the interaction term of TPS with *VolP* (*VolCF*) is very small and insignificant (only marginally significant and small).

Taken together, Tables 5.3 and 5.4 show that the separate view does not yield coherent results, while the TPS-specification produces consistent results. The next subsection investigates if these remarks are robust to applying different TP measures and control settings.

5.5.1.2. Robustness of Both Views

Measuring Tax Planning

To ensure that the baseline results are not driven by arbitrary choices of how to measure TP and TU, I rerun the baseline regressions using the *CETR* and different time horizons over which the proxies are calculated (5, 8, and 10 years). In addition, I use the 5-year variants of book tax differences (BTD), their permanent component ($PBTD$), and the measure of Henry and Sansing (2018) (D_MVA), basing these measures on both income tax expense (GAAP) and cash taxes (Cash). Table 5.5 presents the results for the separate view, while Table 5.6 contains the TPS model. All specifications include firm fixed effects, year fixed effects, and the full set of control variables. Note that fewer observations are available for the 8- and 10-year variants because additional years are needed to perform the rolling window calculations. The baseline conclusions for the separate view are not sensitive to the measure used:

Almost all specifications with ETRs (Panel A) do not yield coefficients with a consistent sign, nor is the main interaction statistically significant in the expected way. The only measure that yields results consistent with Drake, Lusch, and Stekelberg (2019) is BTD

[13] When regressing the TPS on FV without interactions, the coefficient is positive and highly significant throughout all models (see Table B.5.1), which confirms the illustrative graphical representation in Figure 5.1c. Moreover, the positive coefficient for the interaction of TPS and PI in Table 5.4 outweighs the coefficient for TPS, indicating an overall positive relationship.

[14] Hence, if the mean firm increases its *TPS* by one standard deviation, the positive association between a one standard deviation increase in *PI* and the price-to-book ratio increases from 186.77% to 188.26% (186.77%·1.008). As Table 5.6 shows, increasing the TPS by one standard deviation amplifies the positive association between *PI* and FV at most by 4.96% (12.91%, 4.27%, 6.05%) for the GETR (CETR, BTD GAAP, D_MVA) as the basis for TPS, depending on the time horizon.

Table 5.4: Composite View – Baseline

Variable	Base	+Risk	+Controls	Base	+Risk	+Controls
PI	4.955***	4.832***	4.918***	4.769***	4.628***	4.688***
	(0.251)	(0.336)	(0.336)	(0.257)	(0.362)	(0.363)
TPS	-0.004***	-0.006***	-0.006***	-0.005***	-0.007***	-0.007***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
$TPS\#PI$	0.043***	0.041***	0.041***	0.044***	0.036***	0.037***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
$SalesGrowth$	0.014	0.007	0.007	0.014	0.010	0.008
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
$SalesGrowth\#TPS$	-0.000*	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
CoE	0.873***	0.921***	0.919***	0.886***	0.918***	0.925***
	(0.082)	(0.081)	(0.081)	(0.083)	(0.080)	(0.080)
$VolCF$		3.834***	3.812***		2.376*	2.352*
		(1.120)	(1.118)		(1.302)	(1.299)
$TPS\#VolCF$		0.023	0.024		0.070**	0.065**
		(0.020)	(0.020)		(0.027)	(0.027)
$VolP$		0.178	0.188		0.476**	0.453**
		(0.196)	(0.196)		(0.229)	(0.229)
$TPS\#VolP$		0.001	0.001		0.001	0.001
		(0.002)	(0.002)		(0.002)	(0.002)
$PI\#VolCF$		-19.589***	-19.833***		-15.952***	-16.311***
		(3.862)	(3.852)		(4.627)	(4.620)
$PI\#VolP$		5.470***	5.427***		3.963***	4.042***
		(0.661)	(0.661)		(0.687)	(0.686)
$Leverage$			0.091			0.912***
			(0.285)			(0.339)
$Depreciation$			5.968***			5.259**
			(1.740)			(2.162)
Firm FE	No	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,035	2,035	2,035	2,035	2,035	2,035
R-squared	0.425	0.470	0.472	0.426	0.473	0.478

Note: This table reports the results from regression Eq. 5.9 to replicate the composite view by Jacob and Schütt, 2020. The dependent variable is the price-to-book ratio. Columns (1)–(3) include year fixed effects; columns (4)–(6) include year and firm fixed effects. Controls are added in the columns in packages, indicated by the column heading. Standard errors are reported in parentheses. ***, ***, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

Table 5.5: Separate View – Measurement of TP

Panel A: Effective Tax Rates						
Measure	GETR	GETR	GETR	CETR	CETR	CETR
Time Horizon	5y	8y	10y	5y	8y	10y
TP	-0.783	-2.109	-5.559***	-1.239	-2.935	0.900
	(0.787)	(1.309)	(1.737)	(1.178)	(2.642)	(5.034)
TU	-0.299	0.048	-0.689	2.468*	7.132**	2.128
	(0.597)	(0.712)	(1.127)	(1.454)	(3.380)	(6.771)
TP#TU	1.291	1.151	-0.943	5.004	18.402*	8.303
	(1.362)	(2.048)	(3.426)	(3.828)	(9.650)	(19.429)
Observations	2,035	1,408	1,053	1,116	686	445
R-squared	0.421	0.427	0.450	0.478	0.501	0.470
Panel B: Alternative Measures						
Measure	BTD	BTD	PBTD	PBTD	D_MVA	D_MVA
Basis	GAAP	Cash	GAAP	Cash	GAAP	Cash
TP	14.454***	16.401***	4.555	-4.374	-0.004***	-0.004***
	(1.782)	(3.306)	(4.908)	(8.408)	(0.001)	(0.001)
TU	-9.845**	8.498	-3.074	4.148	-0.005**	-0.023***
	(4.468)	(5.472)	(4.570)	(6.977)	(0.003)	(0.006)
TP#TU	-53.444	-41.115	132.458**	128.050*	-0.000***	-0.000***
	(34.094)	(44.613)	(56.395)	(74.524)	(0.000)	(0.000)
Observations	2,035	1,116	742	426	2,035	1,225
R-squared	0.449	0.506	0.532	0.593	0.441	0.486
PI, VolPI	Yes	Yes	Yes	Yes	Yes	Yes
Interactions	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table reports the robustness results from regression Eq. 5.8 when different tax planning measures are applied (see Table 5.1 for definitions). The dependent variable is the price-to-book ratio. Panel A shows effective tax rates with different time horizons, while Panel B reports results for alternative measures which are calculated on a 5 year basis. All specifications include year and firm fixed effects, as well as all control variables from Table 5.3. Standard errors are reported in parentheses. ***, ***, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

based on income tax expense. However, the interaction term is not significant.

In contrast, the coefficients for the interaction term of TPS and PI in Table 5.6 are more stable in terms of their statistical significance. Interestingly, the magnitude of the coefficient tends to be larger when the CETR is used and longer time horizons are applied. When calculating the GETR-based TPS over the same time horizon as Jacob and Schütt (2020) (10 years), the coefficient sizes are similar to their results. A last result worth noting is that applying the Henry and Sansing (2018) measure to the TPS logic yields consistent results (Table 5.6, Panel B), and the coefficients are significant even in the separate model if D_MVA is based on income tax expense (however, the coefficient on TP has the wrong sign). Since losses were included in the calculation of the measure, this could indicate that the presence of past losses may not be a huge concern (Dyreng et al. 2021) if an appropriate measure is used. Overall, the composite view is more robust to the choice of the TP measure, but the results also suggest that the economic size of the association varies across measures.[15]

Control Settings

A major problem in conducting empirical analyses based on conditioning approaches is the choice of control variables. Omitted and unobserved variables that are correlated with the dependent and independent variables could bias the observed associations. While the inclusion of firm fixed effects is commonly used to mitigate this problem, it cannot be completely ruled out. Moreover, prior studies have made different choices about how to measure firm characteristics, and the direction of causality is often ambiguous, since it is not clear whether the independent variables affect the dependent variable or vice versa.[16] As a final robustness check, I run the baseline regressions with 13 different settings oriented on models from previous studies.[17] By comparing how the coefficient estimates vary across different choices how to operationalize broad concepts (e.g., firm size either as total assets, sales, or market value), the robustness of the models can be assessed. Figure 5.2 displays the coefficient estimates for the separate view with the GETR across control settings, while Figure 5.3 shows the composite view. Table A.5.2 (Table A.5.3) in Appendix A shows the point estimates for the separate view (composite view).

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[15] In supplemental analyses in Appendix B, I attempt to reconcile the separate and composite views by translating the logic of the former into the latter and vice versa. Although the estimates from this exercise are difficult to interpret, the results indicate that the separate view performs more robustly when it is applied through income channels. Nevertheless, the notion of Jacob and Schütt (2020), p. 428 regarding potential misspecification is still confirmed.

[16] For example, many of the control variables in the valuation literature, as well as (components of) the price-to-book ratio itself, are often used as independent variables in studies on the determinants of TP (e.g., Mills 1998; Dyreng, Hanlon, and Maydew 2008; 2010). The same applies to control variables calculated in a similar way as the dependent variable (e.g., CoE/CoC when stock price is used as an approximation).

[17] Table A.5.1 provides an overview of the variables used. Due to data limitations, not all variables in the studies could be used. However, the specifications were replicated as closely as possible and show considerable variation in settings.

Table 5.6: Composite View – Measurement of TP

Panel A: TPS Based On Effective Tax Rates						
Measure	GETR	GETR	GETR	CETR	CETR	CETR
Time Horizon	5y	8y	10y	5y	8y	10y
PI	4.688***	2.299***	2.111***	1.820***	2.518**	2.690
	(0.363)	(0.447)	(0.565)	(0.619)	(1.195)	(1.642)
TPS	-0.007***	-0.026***	-0.030***	-0.037***	-0.072***	-0.087*
	(0.002)	(0.005)	(0.008)	(0.008)	(0.024)	(0.048)
$TPS\#PI$	0.037***	0.114***	0.100***	0.203***	0.325***	0.218
	(0.005)	(0.014)	(0.022)	(0.034)	(0.088)	(0.144)
Observations	2,035	1,408	1,053	1,116	686	445
R-squared	0.478	0.500	0.478	0.527	0.571	0.562
Panel B: TPS Based On Alternative Measures						
Measure	BTD	BTD	PBTD	PBTD	D_MVA	D_MVA
Basis	GAAP	Cash	GAAP	Cash	GAAP	Cash
PI	4.383***	2.412***	4.395***	2.137	4.495***	2.569***
	(0.379)	(0.585)	(0.997)	(1.613)	(0.402)	(0.559)
TPS	-0.042***	-0.111***	-0.040	-0.162	-0.005**	-0.028***
	(0.008)	(0.031)	(0.053)	(0.101)	(0.002)	(0.006)
$TPS\#PI$	0.187***	0.689***	0.334	0.994***	0.025***	0.109***
	(0.026)	(0.130)	(0.243)	(0.355)	(0.006)	(0.022)
Observations	2,035	1,116	742	426	2,035	1,225
R-squared	0.471	0.523	0.529	0.620	0.455	0.516
PI, VolPI	Yes	Yes	Yes	Yes	Yes	Yes
Interactions	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table reports the robustness results from regression Eq. 5.9 when different tax planning measures are applied (see Table 5.1 for definitions). The dependent variable is the price-to-book ratio. Panel A shows the results when calculating the TPS based on effective tax rates with different time horizons, while Panel B shows the TPS based on alternative measures which are calculated on a 5 year basis. All specifications include year and firm fixed effects, as well as all control variables from Table 5.4. Standard errors are reported in parentheses. ***, ***, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

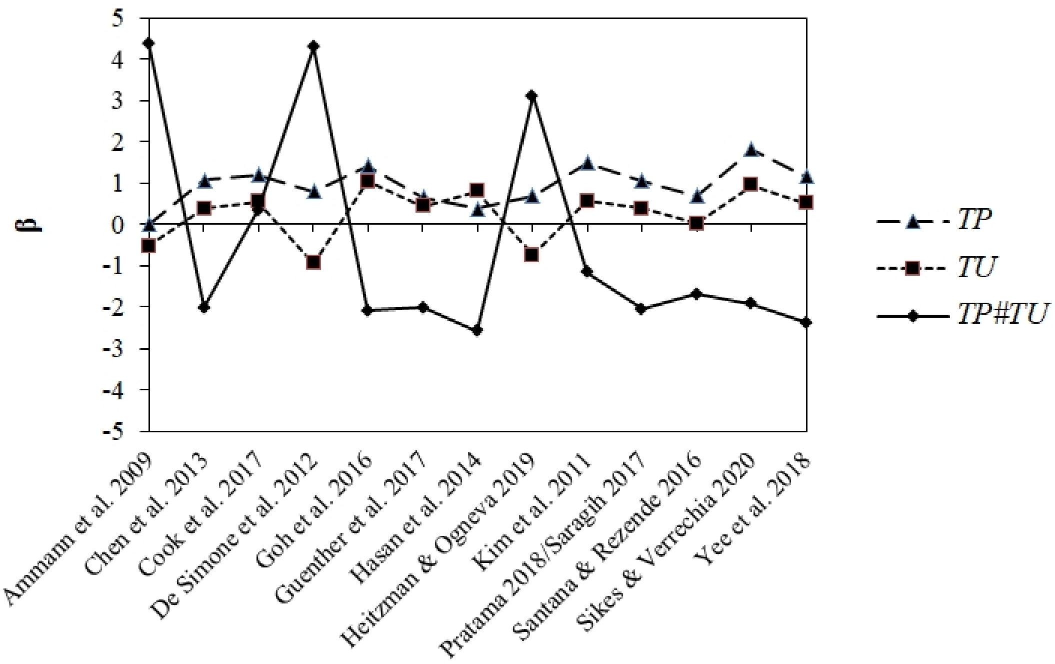

Figure 5.2: Separate View – Control Settings

Note: This figure presents the results from performing regressions for the separate view (Eq. 5.8) with altering control variables using the *GETR*. The x-axis shows the applied control setting (Table A.5.1). Coefficient estimates for *TP*, *TU*, and their interaction are denoted on the y-axis (for point estimates, see Table A.5.2 in Appendix A. All main variables are defined as in the baseline analysis and are described in more detail in Table 5.1.

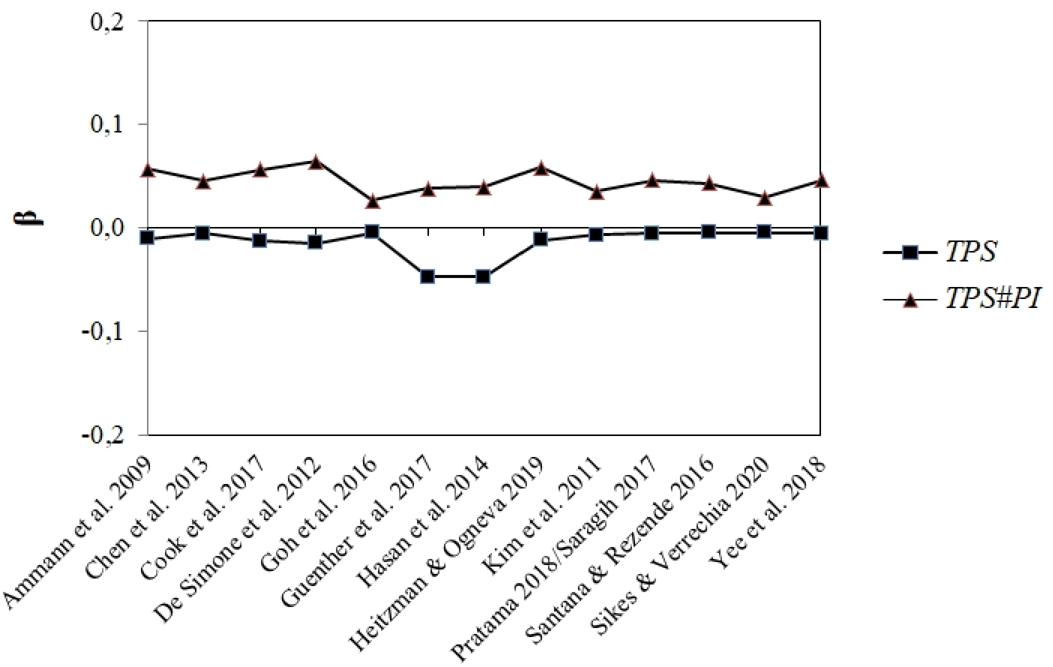

Figure 5.3: Composite View – Control Settings

Note: This figure presents the results from performing regressions for the composite view (Eq. 5.9) with altering control variables using the *TPS* based on the *GETR*. The x-axis shows the applied control setting (Table A.5.1). Coefficient estimates for *PI*, *TPS*, and their interaction are denoted on the y-axis (for point estimates, see Table A.5.3 in Appendix A. All main variables are defined as in the baseline analysis and are described in more detail in Table 5.1.

The estimates for the separate view are spread over a much larger range compared to the TPS specifications. This is especially true for the interaction term between TP and TU. While the coefficients for *TP* and *TU* are relatively stable (although their sign sometimes changes across models), the estimate for the interaction ranges from -2.580 to 4.386. Figure 5.3 shows that the coefficient for the interaction between *TPS* and *PI* ranges only from 0.026 to 0.064. Finally, the coefficients for the separate view are not statistically significant in most cases and only marginally significant in two cases (see Table A.5.2 in Appendix A. For the composite view, all coefficients for the interaction term are significant at the 1% level.

Since all previous analyses indicate that the composite view is better suited to yield consistent results in a valuation framework, the following additional analyses are performed primarily with the TPS and model of Jacob and Schütt (2020).

5.5.2. The Role of Losses: Incidental vs. Incremental TP

Dyreng et al. (2021) point out in a recent working paper that the role of losses has been largely neglected in the TP literature. The robustness tests in Table 5.6 have already shown that the baseline results are robust when the measurement method of Henry and Sansing (2018) is applied to include years with losses in the TPS calculation. However, this analysis was still conducted with the sample that only focuses on firm-years with positive pre-tax income and tax expense to be based on the same sample as the other measures. Therefore, loss years were only included in the calculation of the Henry and Sansing (2018) measure. Dyreng et al. (2021) replicate specifications of selected previous studies and show that their results become insignificant once recent losses are accounted for (e.g., Hasan et al. 2014).

Table 5.7: GETR, D_MVA, TPS, and Losses

Losses in			**GETR**		**D_MVA**		**TPS (GETR)**		**TPS (D_MVA)**	
last 5 years	Obs.	% of Obs.	Mean	Median	Mean	Median	Mean	Median	Mean	Median
0	2,440	50.89%	0.265	0.286	-1.527	-1.172	23.993	10.791	28.97	17.45
1	868	18.10%	0.291	0.272	-0.913	-0.838	17.402	2.722	5.56	3.24
2	564	11.76%	0.263	0.060	0.243	-0.031	17.759	1.891	0.64	0.11
3	402	8.38%	0.124	0.000	1.708	0.907	26.745	2.684	-1.33	-1.26
4	278	5.80%	0.057	0.000	3.628	2.391	18.084	3.608	-2.91	-2.71
5	243	5.07%	0.028	0.000	4.043	3.060	107.434	15.747	-5.22	-4.40
Total	4,795	100%	0.233	0.247	-0.355	-0.822	26.183	5.987	15.28	5.66

Note: This table shows the distribution of the $GETR$, D_MVA, the TPS calculated with the $GETR$, and the TPS based on D_MVA over the number of loss years in the previous 5 years. For each measure and bracket, the mean and median values are reported, along with the number of observations.

Table 5.7 shows the distribution of the mean and median values of the GETR, Delta MVA, the TPS based on the GETR, and on Delta MVA over the number of losses in the last 5 years (0-5). Although the mean GETR does not decrease as monotonically with the number of loss years (when fewer than 2 loss years are documented) as in Dyreng et al. (2021), there is a clear trend that supports the notion that previous loss years are associated with lower GETRs. Without considering this, these values would simply indicate a high level of TP. The values of the TPS based on the GETR do not systematically increase with the number of losses. This could be due to the fact that not only the level of TP but also its volatility is used for calculation, which may partially counteract the random increase of TP. However, the very large number of the TPS (GETR) when there are 5 previous loss years indicates the highest value of uncertainty-adjusted TP when there are the most previous losses, similar to the isolated GETR. The Henry and Sansing (2018) measure, in contrast, shows the opposite pattern: D_MVA (the corresponding TPS) actually increases (decreases) with the number of loss years, implying less (uncertainty-adjusted) TP. This indicates that Delta MVA may not be affected by recent losses in the way that Dyreng et al. (2021) identify as problematic.

The empirical approach of Dyreng et al. (2021) to account for the potential bias of losses is applied to the analyses of this paper by adding the variable $Loss5$ or $Loss5\%$ to the regression Eq. 5.9.[18] $Loss5$ is an indicator variable that equals 1 if pre-tax income was negative in at least one of the previous 5 years, while $Loss5\%$ is the percentage of loss years in the previous 5 years (i.e., if all of the previous 5 years were loss years, this variable would take the value 1).

Table 5.8 reports the results. The first two columns show the separate view with the GETR, while columns 3-4 show the TPS results based on the GETR. The last two columns focus on the analyses that use the Henry and Sansing (2018) measure to calculate the TPS. Table A.5.6 in Appendix A shows the loss analysis for the TPS based on other measures.

Controlling for recent losses seems to affect the results when ETRs are used. The coefficients become insignificant and are essentially zero even when the TPS and the composite view is used. This is consistent with the descriptive evidence in Table 5.7: While the GETRs decrease with the number of recent losses and the GETR-based TPS has the highest value when most of the previous losses are present, in contrast, the TPS based on Henry and Sansing (2018) decreases with the number of previous losses. The key takeaway is that whether the composite view is robust to the distinction between incidental and incremental TP seem to depend on how TP is measured. Table A.5.6 in

[18] When using the loss sample, a problem is that the values of PI are potentially implausible or difficult to interpret when negative values of pre-tax income and common equity are present. In these cases, I set PI to zero and add a control indicator equal to one if PI was affected by this transformation so as not to lose observations. The results and conclusions regarding the TP variables are essentially unchanged (i) without this transformation (but the coefficients on PI become insignificant and sometimes marginally negative), and (ii) when the final estimation sample is restricted to positive values of PI after all other measures have been calculated while including loss years.

Table 5.8: Losses and Incidental vs. Incremental Tax Planning

	GETR		**TPS (GETR)**		**TPS (D_MVA)**	
Variable	Loss5	Loss5%	Loss5	Loss5%	Loss5	Loss5%
PI	2.730***	2.743***	2.085***	2.097***	1.642***	1.657***
	(0.191)	(0.190)	(0.179)	(0.179)	(0.186)	(0.186)
TPS			0.000	0.000	-0.010***	-0.008***
			(0.001)	(0.001)	(0.002)	(0.002)
$TPS\#PI$			-0.001	-0.001	0.051***	0.050***
			(0.001)	(0.001)	(0.007)	(0.007)
TP	0.008	0.008				
	(0.005)	(0.005)				
TU	0.009	-0.002				
	(0.027)	(0.026)				
$TP\#TU$	-0.002	-0.002				
	(0.008)	(0.008)				
$Loss5$	-0.102		-0.106		-0.115	
	(0.080)		(0.080)		(0.082)	
$Loss5\%$		0.314*		0.405*		0.370**
		(0.173)		(0.173)		(0.178)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,795	4,795	4,795	4,795	4,795	4,795
R-squared	0.231	0.207	0.207	0.207	0.218	0.218

Note: This table shows the results when incorporating loss years into the sample. The dependent variable is the price-to-book ratio. Columns 1 and 2 show the results when using the $GETR$ and its uncertainty separately, columns 3 and 4 when using the TPS based on the $GETR$, and columns 5-6 when the Henry and Sansing, 2018 measure is applied to the TPS. Columns 1, 3, 5 (2, 4, 6) include the variable $Loss5$ ($Loss5\%$) which equals one if a firm incurred a loss in at least one of the previous 5 years (which equals the percentage of loss years in the previous 5 years) to control for incidental tax planning due to previous losses (Dyreng et al. 2021). Standard errors are reported in parentheses. ***, ***, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

Appendix A shows that using book tax differences as a basis of the TPS is similarly robust, while using the CETR is again problematic.[19] This supports the results of Dyreng et al. (2021) since they also use ETRs as TP measures (because most of the previous literature did), but it also raises the question whether their conclusions hold for other measurement approaches.

5.5.3. Heterogeneity

As described in Section 5.3.3, the question of what type of firms drive the results, or for which firms the documented positive link between uncertainty-weighted TP, pre-tax income and FV is particularly strong, has not yet been explored. I focus on two possible heterogeneity dimensions: leverage (measured by the total debt to equity ratio, $Leverage$) and available resources, operationalized by two concepts (firm size, measured by market value, $Size$ and liquidity, proxied by cash flows, $Cashflow$[20]).

Regression Eq. 5.9 is performed by splitting the sample into high and low leverage (big and small, high and low cash flow) firms. A firm is considered highly leveraged (big, high cash flow) if it belongs to the top two deciles of each distribution. Table A.5.7 in Appendix A shows the results when median splits are performed. I expect that the link between TP and FV is less pronounced for highly leveraged firms, as default risk becomes more of an issue, and debt overhang could become a problem that affects the equity value (Myers 1977; Cai and Zhang 2011). In addition, the tax benefits from debt-related deductions decrease as TP increases. With respect to available resources, there is no clear prediction. On the one hand, bigger and high cash flow firms tend to have more resources for TP and lower relative planning costs (Eichfelder and Vaillancourt 2014; Hundsdoerfer and Jacob 2019. On the other hand, firms with less resources could derive a larger relative (cash flow) advantage from TP activities.

Table 5.9 shows the results of Eq. 5.9 after splitting the sample, and when interacting $TPS \cdot PI$ with the respective heterogeneity indicator. The sample splits show that the positive association between TPS and PI is statistically significant only for firms with low leverage, small firms, and firms with low cash flows. The only significant interaction term, however, is observed for leverage: the negative sign means that the still significant positive association between the TPS, PI, and FV becomes smaller (and possibly even cancels out) as the leverage becomes higher.

[19]Henry and Sansing (2018), note that their measure is very similar to book tax differences (p. 1052 f.; see also Table 5.1), but negative income tax expense is included. The second difference they highlight is the use of cash taxes paid. To avoid losing too many observations, the main analyses here rely on income tax expense. However, the main conclusions of this paper also apply when D_MVA is calculated on a cash basis.

[20]Since the item cash holdings in Datastream has more missings than cash flows, the latter is used here. Table A.5.8 shows the results when alternative measures are used, i.e., only long term debt for leverage, sales for firm size, and cash holdings for liquidity. The results are qualitatively unchanged.

Table 5.9: Heterogeneity – Leverage, Size, and Liquidity

	Leverage			**Resources**					
	Debt to Equity			**Firm Size**			**Cashflow**		
Variable	High	Low	Interact.	Large	Small	Interact.	High	Low	Interact.
PI	2.910***	5.655***	5.318***	7.490***	4.304***	4.635***	6.163***	4.041***	4.915***
	(0.777)	(0.503)	(0.566)	(1.013)	(0.379)	(0.379)	(0.930)	(0.403)	(0.369)
TPS	0.000	-0.009***	-0.010***	-0.005	-0.006***	-0.005***	-0.009**	-0.006***	-0.005***
	(0.003)	(0.002)	(0.003)	(0.004)	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)
$TPS\#PI$	-0.012	**0.042*** **	0.045***	0.014	**0.036*** **	0.032***	0.019	**0.040*** **	0.035***
	(0.023)	**(0.006)**	(0.007)	(0.013)	**(0.006)**	(0.006)	(0.012)	**(0.006)**	(0.006)
$\#Leverage$			**-0.072** **						
			(0.035)						
$\#Size$						**0.000**			
						(0.001)			
$\#Cashflow$									**0.000**
									(0.001)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	407	1,628	2,035	407	1,628	2,035	407	1,628	2,035
R-squared	0.545	0.487	0.423	0.626	0.495	0.480	0.637	0.490	0.482

Note: This table shows the results from splitting the sample into firms with high/low leverage (columns 1-2), big and small firms (columns 4-5), and firms high high/low cash flow (colums 7-8). Column 3 (5; 7) shows the estimates when interacting $TPS\#PI$ with *Leverage*, as defined as in the baseline (*Size*, measured as the market value; *Cashflow*, measured as the cash flow scaled by total assets). The dependent variable is the price-to-book ratio. Standard errors are reported in parentheses. ***, ***, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

This supports the notion that the higher the debt tax shield, the lower uncertainty-adjusted TP is valued. The results of splitting the sample by size and cash flow can be seen as an indication that the benefits of TP are valued relatively higher in firms for which improving available resources through TP is more important. However, since the interaction terms are not statistically significant, these results should be interpreted with caution. The main conclusions also apply qualitatively when choosing the median as the cut-off to divide the sample (see Table A.5.7 in Appendix A, as well as using alternative measures for the heterogeneity concepts (see Table A.5.8 in Appendix A).

5.6. Conclusion

This paper empirically provides support for the notion of Jacob and Schütt (2020) that TP and TU should be considered jointly in a valuation framework and that their link to FV is better investigated indirectly through income channels. Since the composite view and the TPS have not been as widely recognized in recent studies, this should be taken more seriously in future empirical research. Nonetheless, as the robustness tests have shown, coefficient estimates can vary considerably across measurement choices – even in the composite view. The TPS logic can be extended to a wide range of measures without losing its qualitative robustness, but the quantitative interpretation of results might differ. Therefore, for future empirical studies, it seems advisable to apply different TP measures to interpret more carefully the economic significance of results and not just rely on one arbitrary point estimate. Related to measurement issues, the additional analyses have shown that whether the composite view works well when including loss-making firms and controlling for recent losses depends on the basis of the TPS. Similar to Dyreng et al. (2021), specifications that rely on ETRs appear to be biased by recent losses, while using the Henry and Sansing (2018) measure designed to measure TP in the presence of losses mitigates this problem. Therefore, while Dyreng et al. (2021) conclude that recent losses likely affect the conclusions drawn from TP analyses, this may depend on the careful choice of the TP measure. To confirm and generalize this, however, a comprehensive replication of previous studies similar to Dyreng et al. (2021) with different measurement approaches is needed. The measure of Henry and Sansing (2018) seems to be a promising candidate for this exercise, either in isolation (e.g., for replication of Hasan et al. 2014) or also as the basis of the TPS when uncertainty of TP needs to be accounted for (e.g., Sikes and Verrecchia 2020).

A potential reason that studies have not picked up on the TPS could be that its use does not come without caveats. By applying a composite measure, the incremental impact of TP and TU cannot be properly assessed. Further research is needed in this regard, as the simulations in Jacob and Schütt (2020) and the empirical results of this paper suggest that simply separating the two concepts in standard conditioning approaches risks a strong dependence on measurement and control setting choices (see also Appendix B. Nevertheless, the robustness and additional analyses with the TPS-based specifications indicate that the use of the TPS may be beneficial for future empirical studies on the role of corporate TP not only in valuation but also in other areas of business economics, such as the capital structure choice of firms (Faccio and Xu 2015) – as the heterogeneity analyses suggest that leverage can have an impact on how positively uncertainty-weighted TP is valued – as well as the determinants of the (equity) cost of capital (Cook, Moser, and Omer 2017), or stock returns (Heitzman and Ogneva 2019), where TU has not yet been explicitly considered.

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Compliance with Ethical Standards

Competing Interests

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Research Data Policy and Data Availability Statements

The dataset generated for the current study stems from Datastream by Thomson Reuters and is not publicly available without a user's license. Information on how to obtain it and reproduce the analysis is available from the author on request, including statistical code.

Appendices

Appendix A: Further Statistics and Robustness Tests

A.1 Point Estimates Dependent on Control Settings

Table A.5.1 shows the various control settings used to create tables A.5.2 and A.5.3, the results of which are shown in the main paper in Figures 5.2 (for the separate view) and 5.3 (for the composite view). The settings are oriented on previous studies that touch on the main issues of the paper, and vary considerably. In some cases, the settings were adjusted because the focus of the selected studies deviated slightly from directly examining the value implications of TP (e.g., the book-to-market ratio was not included because the inverse of this measure is the dependent variable).

Table A.5.4 (A.5.5) contains the results of the same exercise when the CETR is used instead of the GETR as the TP measure for the separate (composite) view. In general, the observations from the GETR analyses also apply to the CETR. When using the latter, the coefficient estimates become even more unstable, as shown in Figures A.5.1 and A.5.2.

Table A.5.1: Control variables oriented on prior literature

Oriented on	**Control Variables**
Ammann, Oesch, and Schmid, 2011	Total Assets; Property, Plant and Equipment; Leverage; Ebitda; Cash; Sales Growth; Research and Development; Capital Expenditures
Chen et al., 2014	Total Assets; Property, Plant and Equipment; Leverage; Sales Growth; Beta; Return on Assets
Cook, Moser, and Omer, 2017	Total Assets; Property, Plant and Equipment; Dividend Yield; Leverage; Sales Growth; Return on Assets; Price Volatility; Ebitda Volatility; Research and Development; Capital Expenditures
De Simone and Stomberg, 2012	Sales; Sales Growth; Leverage; Return on Assets; Price Volatility; Research and Development; Capital Expenditures
Goh et al., 2016	Market Value; Leverage; Ebitda; Sales Growth; Beta; Stock Return; Price Volatility; Ebitda Volatility; Capital Expenditures
Guenther, Matsunaga, and Williams, 2017	Total Assets; Leverage; Ebitda; Ebitda Volatility; Cashflow Volatility
Hasan et al., 2014	Total Assets; Leverage; Sales Growth; Return on Assets; Cash; Property, Plant and Equipment; Ebitda Ebitda Volatility
Heitzman and Ogneva, 2019	Total Assets; Property, Plant and Equipment; Leverage; Stock Return; Price Volatility; Research and Development; Capital Expenditures
Kim, Li, and Zhang, 2011	Market Value; Sales; Leverage; Return on Assets; Stock Return; Price Volatility
Pratama, 2018; Saragih, 2017	Total Assets; Leverage; Return on Assets
Santana and Rezende, 2016	Sales; Property, Plant and Equipment; Long Term Debt; Cashflow
Sikes and Verrecchia, 2020	Market Value, Leverage; Return on Equity; Beta; Dividend Yield
Yee, Sapiei, and Abdullah, 2018	Total Assets; Leverage; Sales Growth; Return on Assets; Return on Equity

Table A.5.2: Separate View – Control Settings

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
TP	-0.001	-1.075	-1.211	-0.811	-1.419**	-0.659	-0.391
	(1.333)	(0.807)	(1.217)	(1.282)	(0.679)	(0.836)	(0.866)
TU	-0.515	0.382	0.554	-0.939	1.034**	0.446	0.799
	(1.125)	(0.618)	(1.015)	(1.077)	(0.505)	(0.618)	(0.641)
$TP\#TU$	-4.386	2.015	-0.346	-4.308	2.098*	2.024	2.580
	(2.787)	(1.546)	(2.511)	(2.668)	(1.264)	(1.548)	(1.614)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	915	2,033	906	942	1,941	1,941	1,853
R-squared	0.331	0.230	0.469	0.404	0.483	0.219	0.231

Variable	(8)	(9)	(10)	(11)	(12)	(13)
TP	-0.696	-1.493**	-1.058	-0.702	-1.824***	-1.166
	(1.317)	(0.672)	(0.808)	(0.824)	(0.621)	(0.735)
TU	-0.734	0.572	0.390	0.014	0.956**	0.520
	(1.102)	(0.515)	(0.617)	(0.630)	(0.474)	(0.561)
$TP\#TU$	-3.113	1.159	2.061	1.683	1.922	2.380*
	(2.739)	(1.287)	(1.543)	(1.577)	(1.187)	(1.405)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	942	2,035	2,035	2,024	2,035	2,035
R-squared	0.378	0.466	0.226	0.196	0.544	0.360

Note: This table reports the point estimates that are shown in Figure 5.2 for regression Eq. 5.8 with different control settings. The dependent variable is the price-to-book ratio, while TP and TU are based on the $GETR$. Column (1) refers to the control setting following Ammann, Oesch, and Schmid, 2011, column (2) Chen et al., 2014, column (3) Cook, Moser, and Omer, 2017, column (4) De Simone and Stomberg, 2012, column (5) Goh et al., 2016, column (6) Guenther, Matsunaga, and Williams, 2017, column (7) Hasan et al., 2014, column (8) Heitzman and Ogneva, 2019, column (9) Kim, Li, and Zhang, 2011, column (10) Pratama, 2018; Saragih, 2017, column (11) Santana and Rezende, 2016, column (12) Sikes and Verrecchia, 2020, and column (13) Yee, Sapiei, and Abdullah, 2018. All specifications include year and firm fixed effects. Standard errors are reported in parentheses. ***, ***, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

Table A.5.3: Composite View – Control Settings

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
PI	6.898***	5.895***	5.574***	6.613***	5.655***	5.549***	5.931***
	(0.531)	(0.338)	(0.521)	(0.533)	(0.316)	(0.329)	(0.348)
TPS	-0.011***	-0.006***	-0.013***	-0.015***	-0.005***	-0.005**	-0.005**
	(0.004)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
$TPS\#PI$	0.057***	0.045***	0.056***	0.064***	0.026***	0.038***	0.039***
	(0.010)	(0.006)	(0.010)	(0.010)	(0.005)	(0.006)	(0.006)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	915	2,033	906	942	1,941	1,941	1,853
R-squared	0.477	0.393	0.560	0.524	0.584	0.379	0.393

Variable	(8)	(9)	(10)	(11)	(12)	(13)
PI	5.348***	5.736***	5.911***	5.092***	4.124***	5.914***
	(0.384)	(0.274)	(0.337)	(0.279)	(0.228)	(0.338)
TPS	-0.012***	-0.007***	-0.006***	-0.005***	-0.005***	-0.006***
	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
$TPS\#PI$	0.058***	0.035***	0.046***	0.043***	0.029***	0.046***
	(0.009)	(0.005)	(0.006)	(0.006)	(0.005)	(0.006)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	942	2,035	2,035	2,024	2,035	2,035
R-squared	0.557	0.599	0.392	0.388	0.551	0.392

Note: This table reports the point estimates that are shown in Figure 5.3 for regression Eq. 5.9 with different control settings. The dependent variable is the price-to-book ratio, while the TPS is based on the $GETR$. Column (1) refers to the control setting following Ammann, Oesch, and Schmid, 2011, column (2) Chen et al., 2014, column (3) Cook, Moser, and Omer, 2017, column (4) De Simone and Stomberg, 2012, column (5) Goh et al., 2016, column (6) Guenther, Matsunaga, and Williams, 2017, column (7) Hasan et al., 2014, column (8) Heitzman and Ogneva, 2019, column (9) Kim, Li, and Zhang, 2011, column (10) Pratama, 2018; Saragih, 2017, column (11) Santana and Rezende, 2016, column (12) Sikes and Verrecchia, 2020, and column (13) Yee, Sapiei, and Abdullah, 2018. All specifications include year and firm fixed effects. Standard errors are reported in parentheses. ***, ***, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

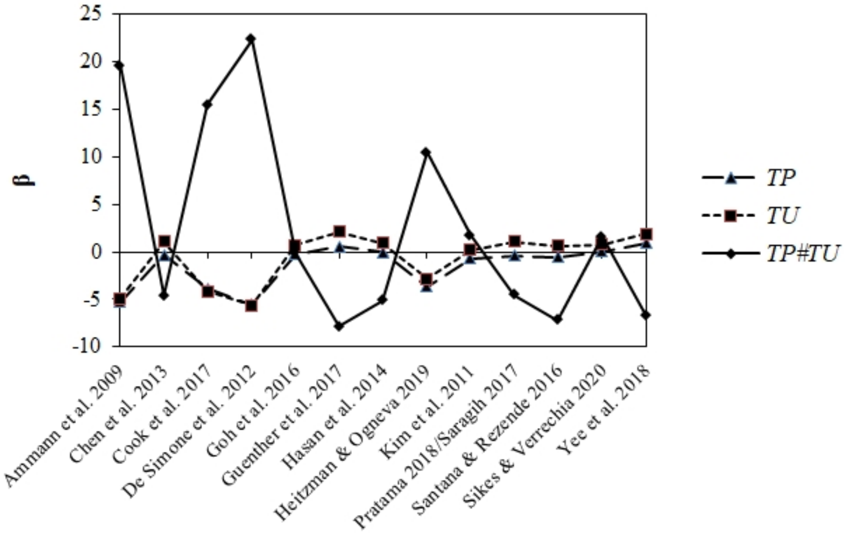

Figure A.5.1: Separate View CETR – Control Settings

Note: This figure presents the results from performing regressions for the separate view (Eq. 5.8) with altering control variables using the $CETR$. The x-axis shows the applied control setting (Table A.5.1). Coefficient estimates for *TP*, *TU*, and their interaction are denoted on the y-axis (Table A.5.4 in Appendix A). All main variables are defined as in the baseline analysis and are described in more detail in Table 5.1.

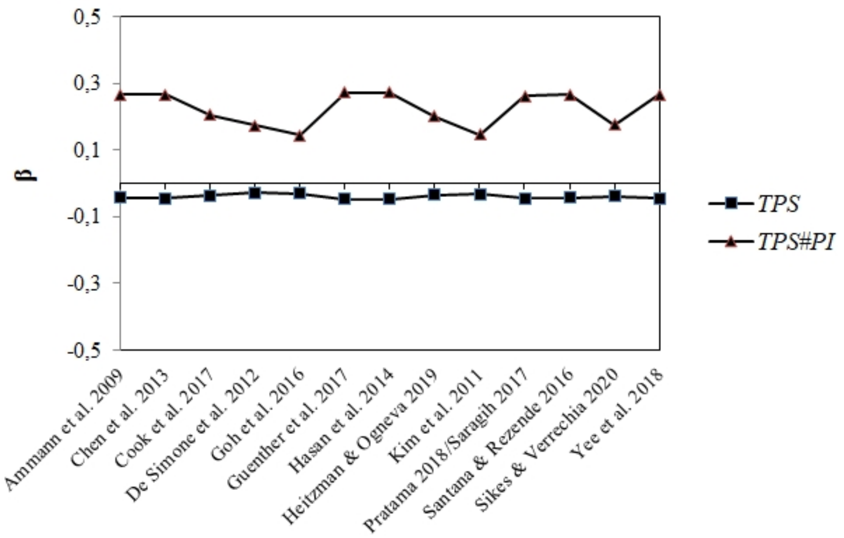

Figure A.5.2: Composite View CETR – Control Settings

Note: This figure presents the results from performing regressions for the composite view (Eq. 5.9) with altering control variables using the TPS based on the $CETR$. The x-axis shows the applied control setting (Table A.5.1). Coefficient estimates for *TPS*, and the interaction with *PI* are denoted on the y-axis (Table A.5.5 in Appendix A. All main variables are defined as in the baseline analysis and are described in more detail in Table 5.1.

Table A.5.4: Separate View CETR – Control Settings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
TP	5.230***	0.287	3.869**	5.523***	0.213	-0.602	0.060
	(1.896)	(1.217)	(1.580)	(1.675)	(0.952)	(1.203)	(1.276)
TU	-4.860**	1.073	-4.156**	-5.636***	0.709	2.121	0.914
	(2.181)	(1.418)	(1.848)	(1.959)	(1.100)	(1.393)	(1.469)
$TP\#TU$	-19.599***	4.630	-15.477**	-22.351***	0.081	7.831*	5.108
	(7.192)	(4.348)	(6.006)	(6.444)	(3.379)	(4.241)	(4.522)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	633	1,116	618	641	1,075	1,075	1,064
R-squared	0.318	0.303	0.536	0.424	0.613	0.374	0.312

	(8)	(9)	(10)	(11)	(12)	(13)
TP	3.650**	0.665	0.409	0.521	-0.076	-0.901
	(1.515)	(0.926)	(1.219)	(1.276)	(0.898)	(1.169)
TU	-2.803	0.165	1.049	0.630	0.762	1.911
	(1.765)	(1.082)	(1.421)	(1.498)	(1.044)	(1.357)
$TP\#TU$	-10.437*	-1.748	4.507	7.200	-1.567	6.663
	(5.810)	(3.322)	(4.354)	(4.552)	(3.204)	(4.156)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	641	1,116	1,116	1,116	1,116	1,116
R-squared	0.535	0.598	0.296	0.230	0.626	0.363

Note: The dependent variable is the price-to-book ratio. Column (1) refers to the control setting following Ammann, Oesch, and Schmid, 2011, column (2) Chen et al., 2014, column (3) Cook, Moser, and Omer, 2017, column (4) De Simone and Stomberg, 2012, column (5) Goh et al., 2016, column (6) Guenther, Matsunaga, and Williams, 2017, column (7) Hasan et al., 2014, column (8) Heitzman and Ogneva, 2019, column (9) Kim, Li, and Zhang, 2011, column (10) Pratama, 2018; Saragih, 2017, column (11) Santana and Rezende, 2016, column (12) Sikes and Verrecchia, 2020, and column (13) Yee, Sapiei, and Abdullah, 2018. All specifications include year and firm fixed effects. Standard errors are reported in parentheses. ***, ***, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

Table A.5.5: Composite View CETR – Control Settings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
PI	3.598***	1.950***	1.038	3.350***	3.484***	0.152	1.754***
	(1.022)	(0.594)	(0.956)	(0.976)	(0.574)	(0.676)	(0.615)
TPS	-0.044***	-0.046***	-0.036***	-0.028*	-0.031***	-0.048***	-0.047***
	(0.015)	(0.008)	(0.014)	(0.014)	(0.007)	(0.009)	(0.009)
$TPS\#PI$	0.267***	0.265***	0.204***	0.173***	0.143***	0.273***	0.273***
	(0.060)	(0.034)	(0.054)	(0.057)	(0.027)	(0.034)	(0.035)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	633	1,116	618	641	1,075	1,075	1,064
R-squared	0.393	0.412	0.556	0.455	0.668	0.437	0.420

	(8)	(9)	(10)	(11)	(12)	(13)
PI	1.295*	2.569***	1.997***	2.534***	1.981***	1.983***
	(0.663)	(0.454)	(0.595)	(0.510)	(0.419)	(0.595)
TPS	-0.035***	-0.032***	-0.045***	-0.043***	-0.038***	-0.046***
	(0.013)	(0.006)	(0.008)	(0.009)	(0.007)	(0.008)
$TPS\#PI$	0.201***	0.147***	0.262***	0.267***	0.175***	0.267***
	(0.051)	(0.026)	(0.034)	(0.034)	(0.027)	(0.034)
Firm FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Observations	641	1,116	1,116	1,116	1,116	1,116
R-squared	0.573	0.659	0.407	0.387	0.639	0.408

Note: The dependent variable is the price-to-book ratio. Column (1) refers to the control setting following Ammann, Oesch, and Schmid, 2011, column (2) Chen et al., 2014, column (3) Cook, Moser, and Omer, 2017, column (4) De Simone and Stomberg, 2012, column (5) Goh et al., 2016, column (6) Guenther, Matsunaga, and Williams, 2017, column (7) Hasan et al., 2014, column (8) Heitzman and Ogneva, 2019, column (9) Kim, Li, and Zhang, 2011, column (10) Pratama, 2018; Saragih, 2017, column (11) Santana and Rezende, 2016, column (12) Sikes and Verrecchia, 2020, and column (13) Yee, Sapiei, and Abdullah, 2018. All specifications include year and firm fixed effects. Standard errors are reported in parentheses. ***, ***, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

A.2 Robustness for Loss Analysis

Table A.5.6 shows the results for the loss analyses (Section 5.5.2 in the paper) when using the TPS based on CETRs (columns 1 and 2), the TPS based on BTD (columns 3 and 4), and the TPS based on PBTD (columns 5 and 6).

Controlling for recent losses as proposed by Dyreng et al. (2021) ($Loss5$) seems to only affect the baseline results when effective tax rates are used as a measure, confirming the results in the main paper on the GETR. The coefficients become insignificant and essentially zero, even if the TPS and the composite view is used. Using book tax differences leads to results that are not substantially altered (see also Table 5.6). As Henry and Sansing (2018) note, their measure is close to book tax differences, but slightly different (they find a positive correlation between D_MVA and BTD of about 0.8). Overall, TP measures need to be carefully chosen – even in the composite view – to account for recent losses.

Table A.5.6: Losses Robustness – TPS on CETR and Book-Tax-Differences

	TPS (CETR)		**TPS (BTD)**		**TPS (PBTD)**	
Variable	Loss5	Loss5%	Loss5	Loss5%	Loss5	Loss5%
PI	2.433***	2.426***	1.565***	1.576***	0.554	0.606
	(0.234)	(0.234)	(0.183)	(0.183)	(0.494)	(0.494)
TPS	0.001	0.001	-0.063***	-0.057***	-0.116**	-0.098*
	(0.001)	(0.001)	(0.012)	(0.012)	(0.051)	(0.052)
$TPS\#PI$	-0.002	-0.002	0.368***	0.361***	1.160***	1.116***
	(0.003)	(0.003)	(0.037)	(0.037)	(0.218)	(0.219)
$Loss5$	0.069		-0.098		-0.147	
	(0.088)		(0.082)		(0.150)	
$Loss5\%$		0.203		0.349**		0.372
		(0.217)		(0.177)		(0.417)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,190	2,190	4,795	4,795	1,262	1,262
R-squared	0.270	0.270	0.228	0.228	0.467	0.467

Note: This table shows the robustness results for the loss analyses as suggested by Dyreng et al., 2021 in Section 5.5.2. The dependent variable is the price-to-book ratio. Columns 1 and 2 show the results when calculating the TPS based on CETRs, columns 3 and 4 when it is calculated with BTD, and columns 5 and 6 when it is based on $PBTD$. Columns 1, 3, 5 (2, 4, 6) include the variable $Loss5$ ($Loss5\%$). Standard errors are reported in parentheses. ***, ***, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

A.3 Robustness for Heterogeneity Analyses

Table A.5.7 shows the results for choosing the median of the heterogeneity indicator as cutoff for the sample splits instead of 20/80 splits as in Table 5.9 in the main paper. The conclusions from the main text hold: The positive association between pre-tax income, the TPS, and the price-to-book ratio is statistically significant only in firms with low leverage, while the coefficients on the interaction term of TPS and PI are slightly larger for relatively small firms and firms with low cash flows.

Table A.5.8 shows the results for applying alternative measurement approaches to the heterogeneity dimensions: Leverage is operationalized by using long term debt only, firm size is measured by sales, and cash holdings are used instead of cash flows. As this item has a few more missings than the other variables, the observation number slightly differs from the baseline sample (1,913 observations instead of 2,035).

Table A.5.7: Robustness Heterogeneity – Median Splits

	Leverage		**Resources**			
Variable	High	Low	Large	Small	High CF	Low CF
PI	4.616***	3.471***	4.385***	3.183***	4.404***	4.853***
	(0.531)	(0.622)	(0.566)	(1.013)	(0.531)	(0.557)
TPS	-0.001	-0.012***	-0.005**	-0.007***	-0.008***	-0.006**
	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)
$TPS\#PI$	0.004	**0.047*** **	0.026***	**0.059*** **	0.029***	**0.042*** **
	(0.012)	**(0.006)**	(0.008)	**(0.009)**	(0.009)	**(0.007)**
Obs.	1,018	1,017	1,018	1,017	1,018	1,017
R-squared	0.460	0.537	0.489	0.354	0.478	0.495
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table shows the results from splitting the sample at median values (instead of top 2 deciles vs. all other deciles as in Table 5.9) into firms with high/low leverage (columns 1-2), big and small firms (columns 3-4), and firms high high/low cash flows (colums 5-6). The dependent variable is the price-to-book ratio. Standard errors are reported in parentheses. ***, ***, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

Table A.5.8: Robustness Heterogeneity – Long Term Debt, Sales, and Cash Holdings

	Leverage			**Resources**					
	Long Term Debt			**Firm Size (Sales)**			**Cash Holdings**		
Variable	High	Low	Interact.	Large	Small	Interact.	High	Low	Interact.
PI	5.018***	4.148***	4.434***	9.450***	2.739***	2.281***	3.591**	4.965***	4.668***
	(0.988)	(0.480)	(0.489)	(0.793)	(0.412)	(0.535)	(1.467)	(0.357)	(0.345)
TPS	0.001	-0.011***	-0.010***	0.003	-0.008***	-0.008***	-0.023***	-0.003**	-0.006***
	(0.004)	(0.002)	(0.002)	(0.004)	(0.002)	(0.003)	(0.008)	(0.002)	(0.002)
TPS#PI	-0.006	**0.048*** **	0.043***	0.021**	**0.039*** **	0.044***	0.027	**0.023*** **	0.026***
	(0.024)	**(0.006)**	(0.006)	(0.009)	**(0.008)**	(0.013)	(0.017)	**(0.006)**	(0.008)
#Leverage			**-0.025**						
			(0.044)						
#Size						**-0.007**			
						(0.007)			
#Cash									**-0.005**
									(0.027)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	407	1,628	2,035	407	1,628	2,035	383	1,530	1,913
R-squared	0.576	0.452	0.436	0.659	0.453	0.480	0.582	0.501	0.489

Note: This table shows the results from splitting the sample into firms with high/low leverage (columns 1-2), big and small firms (columns 4-5), and firms high high/low cash holdings instead of cashflows as in Table 5.9 (colums 7-8). Column 3 (5; 7) shows the estimates when interacting *TPS#PI* with *Leverage*, using long-term debt (*Size*, measured as sales; *Cash*, measured as the cash holdings). The dependent variable is the price-to-book ratio. Standard errors are reported in parentheses. ***, ***, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

Appendix B: Reconciling the Separate and Composite Views

Despite the different view on the consideration of TU, Drake, Lusch, and Stekelberg (2019) assume a direct link between TP and FV without interactions with pre-tax income, as does Jacob and Schütt (2020). When using the TPS as a measure of (uncertainty weighted) TP, the separate view would imply a direct link between TPS and FV. Accordingly, the TPS can be inserted into Eq. 5.8 without interaction terms, yielding the following:

$$\begin{aligned} PTB_{i,t} = \beta_0 + \beta_1 TPS_{i,t} + \beta_4 PI_{i,t} + \beta_5 VolPI_{i,t} + \beta_6 PI_{i,t} \cdot VolPI_{i,t} \\ + \beta_6 SalesGrowth_{i,t} + \beta_7 X_{i,t} + \theta_i + \gamma_t + \epsilon_{i,t}, \end{aligned} \tag{B.5.1}$$

where β_1 is expected to be positive, since a higher TPS indicates either a higher level or a lower volatility of TP (or both).[21]

Table B.5.1: TPS – Isolated

Variable	(1)	(2)	(3)	(4)	(5)	(6)
PI	6.080***	7.132***	7.344***	5.812***	6.701***	7.021***
	(0.249)	(0.438)	(0.441)	(0.256)	(0.489)	(0.496)
TPS	0.006***	0.004***	0.004***	0.006***	0.004***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Risk Controls	No	Yes	Yes	No	Yes	Yes
Other Controls	No	No	Yes	No	No	Yes
Firm FE	No	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,035	2,035	2,035	2,035	2,035	2,035
R-squared	0.364	0.414	0.417	0.365	0.417	0.422

Note: This table reports the results of estimating Eq. B.5.1. The TPS is calculated using the GETR over 5 years as in the baseline analyses. The dependent variable is the price-to-book ratio. Columns (1)–(3) include year fixed effects; columns (4)–(6) include year and firm fixed effects. Standard errors are reported in parentheses. ***, ***, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

Table B.5.1 contains the results of estimating Eq. B.5.1. As predicted, the coefficient estimates in all columns are positive and statistically significant at the 1%-level (see also Figure 5.1c).

A general drawback of the TPS by construction is that one cannot be sure how a certain TPS value was achieved by a firm. For example, a firm with a TPS of 10 can either have an ETR of 0.3 and a corresponding volatility of 7%, or an ETR of 0.5 with a volatility of 5%.[22] By only looking at the TPS, it is unclear whether the degree of

[21] Note that Eq. 5.3 makes no prediction about the direct effect of the TPS on FV. However, the intuition behind the Jacob and Schütt (2020) model would imply that the TPS should be positively associated with FV. This is also supported by Figure 5.1, Panel 5.1c in the main paper.

[22] Calculated using the TPS formula of Eq. 5.7: $(1 - 0.3)/0.07 = (1 - 0.5)/0.05 = 10$

TP (numerator) or rather the TU (denominator) is the main driver behind potential FV associations. To gain more insight into this, the indirect connection of TP and FV through income channels can be modeled in the separate view by interacting TP and TU with PI and adjusting Eq. 5.8.[23] For brevity, the control vector X contains all the baseline control variables previously used in all following equations. For the sake of interpretation, I omit the triple interaction term:

$$PTB_{i,t} = \beta_0 + \beta_1 PI_{i,t} + \beta_2 TP_{i,t} + \beta_3 TU_{i,t} + \beta_4 PI_{i,t} \cdot TP_{i,t} + \beta_5 PI_{i,t} \cdot TU_{i,t} + \beta_6 X_{i,t} + \theta_i + \gamma_t + \epsilon_{i,t}, \tag{B.5.2}$$

In Eq. B.5.2, β_2 and β_4 would be expected to be positive (reflecting a positive association between the level of TP and FV, as well as an amplifying impact of TP on the coefficient of PI), while β_3 and β_4 would be expected to be negative. Table B.5.2 contains the results of estimating Eq. B.5.2.

Table B.5.2: Separate View – Double Interaction with PI

Variable	(1)	(2)	(3)	(4)	(5)	(6)
PI	2.725***	5.410***	5.643***	2.785***	4.519***	4.739***
	(0.568)	(0.842)	(0.843)	(0.594)	(0.881)	(0.883)
TP	1.665***	0.665	0.810	1.531**	0.606	0.606
	(0.613)	(0.648)	(0.647)	(0.697)	(0.746)	(0.744)
TU	-0.144	-0.260	-0.219	-0.237	-0.673**	-0.676**
	(0.236)	(0.265)	(0.264)	(0.241)	(0.271)	(0.270)
$TP\#PI$	-13.591***	-6.590***	-6.446***	-12.117***	-8.562***	-8.894***
	(1.994)	(2.304)	(2.305)	(2.086)	(2.426)	(2.423)
$TU\#PI$	-1.364**	-5.668***	-5.612***	-1.155*	-6.170***	-6.224***
	(0.688)	(1.021)	(1.020)	(0.694)	(1.039)	(1.036)
Risk Controls	No	Yes	Yes	No	Yes	Yes
Other Controls	No	No	Yes	No	No	Yes
Firm FE	No	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,035	2,035	2,035	2,035	2,035	2,035
R-squared	0.368	0.425	0.427	0.369	0.430	0.435

Note: This table reports the results from estimating Eq. B.5.2 similar to Jacob and Schütt, 2020, p. 428, Table 6. The dependent variable is the price-to-book ratio. Columns (1)–(3) include year fixed effects; columns (4)–(6) include year and firm fixed effects. Standard errors are reported in parantheses. ***, ***, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

A comparison with the baseline replication of the separate view (Table 5.3 and the robustness tests in the main paper) shows that the coefficients for TP and TU have the expected sign and are more stable (although TP is significant only in columns 1 and 4 and

[23] As Jacob and Schütt (2020) have already indicated, and as the replications in the main paper have shown, however, this is precisely the kind of specification that is prone to misspecification. Nevertheless, separately measuring TP and TU while indirectly connecting them to FV via income is a suitable final robustness check for this notion (see also Table 6 in Jacob and Schütt 2020, p. 428).

TU only in columns 5 and 6). The interactions with PI are significant in all columns, but the estimates for $TP\#PI$ are negative, implying that the higher the GETR, the larger the positive association between pre-tax income and FV. Lastly, interacting TP and TU with PI yields a slightly higher explanatory power of the models compared to directly linking TP and TU to FV. However, the conclusions that can be drawn from this exercise are limited. This is also supported by Table B.5.3, which includes the triple interaction term between TP, TU, and PI.

Table B.5.3: Separate View – Tripe Interaction With PI

Variable	(1)	(2)	(3)	(4)	(5)	(6)
PI	1.778**	5.157***	5.416***	1.610**	3.710***	3.964***
	(0.751)	(0.974)	(0.975)	(0.789)	(1.019)	(1.021)
TP	2.097***	1.012	1.161	1.596*	1.050	1.068
	(0.774)	(0.791)	(0.790)	(0.888)	(0.923)	(0.920)
TU	-0.634	-0.717	-0.696	-0.346	-1.065	-1.101
	(0.666)	(0.665)	(0.665)	(0.687)	(0.686)	(0.685)
$TP\#TU$	-1.448	-1.231	-1.270	-0.443	-1.199	-1.274
	(1.679)	(1.619)	(1.617)	(1.748)	(1.686)	(1.682)
$TP\#PI$	-16.926***	-7.454***	-7.217**	-16.170***	-11.510***	-11.701***
	(2.611)	(2.839)	(2.837)	(2.744)	(3.049)	(3.043)
$TU\#PI$	2.954	-4.634*	-4.734*	4.060*	-2.451	-2.704
	(2.302)	(2.465)	(2.462)	(2.318)	(2.509)	(2.502)
$TP\#TU\#PI$	11.832**	2.888	2.504	14.310**	9.656	9.172
	(5.994)	(5.874)	(5.867)	(6.059)	(5.983)	(5.966)
Risk Controls	No	Yes	Yes	No	Yes	Yes
Other Controls	No	No	Yes	No	No	Yes
Firm FE	No	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,035	2,035	2,035	2,035	2,035	2,035
R-squared	0.370	0.425	0.427	0.372	0.431	0.436

Note: This table reports the results from Eq. B.5.2 when the triple interaction of TP, TU, and PI is included. The dependent variable is the price-to-book ratio. Columns (1)–(3) include year fixed effects; columns (4)–(6) include year and firm fixed effects. Standard errors are reported in parantheses. ***, ***, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

Compared to directly relating TP and TU to FV, it seems that following the residual income model as a theoretical background by interacting TP and TU with pre-tax income leads to more consistent results – even in the separate view: The coefficients for TP (TU) in Table B.5.3 are positive (negative), while the interaction of TP and TU is negative, as would be expected by the separate view. While the remarks regarding the size and signs of estimates for $TP\#PI$ and $TU\#PI$ from Table B.5.2 still hold, the most important triple interaction is positive, contrary to expectations. Moreover, the estimates are significant only in columns 1 and 4 and are very difficult to interpret economically due to the large number of interactions involved.

Overall, reconciling the separate and composite view confirms the findings from the main paper and from Jacob and Schütt (2020): (i) The composite view leads to more stable and consistent empirical results than the separate view. (ii) Measuring TP and TU separately but interacting them with pre-tax income leads to results that are slightly more stable and more consistent with Drake, Lusch, and Stekelberg (2019), but dependence on modeling decisions remains and interpretation of estimates becomes difficult. (iii) Despite the disadvantage of a composite measure of not being able to be sure whether the degree of TP or rather its uncertainty is more important in a valuation framework, the idea that they should be considered jointly seems to be the most consistent choice. The estimates in Tables B.5.2 and B.5.3 do not allow a reasoned conclusion as to whether TP or TU is more important because either there is no statistical significance or the gap between estimates for the two concepts is not significantly different (e.g., column 6 of Table B.5.2).

Chapter 6

Wage Response to Corporate Income Taxes: A Meta-Regression Analysis

Wage Response to Corporate Income Taxes: A Meta-Regression Analysis

Abstract

The wage elasticity to corporate income tax (CIT) is an essential parameter for assessing tax policy reforms. This paper applies meta-regression analysis to quantitatively review the growing empirical tax incidence literature that indicates a substantial shift of the tax burden onto employees. While most studies report a large wage-reducing effect of the CIT, our findings suggest that estimates with positive values are published less often than they should. After accounting for the bias, we find no significant average association between wage rates and corporate taxation. We document that the tax variable, econometric method, type of tax variation, and underlying time and country coverage of studies drive the heterogeneity among reported effects. The implied best-practice estimates suggest that the tax elasticity of wages is systematically larger for emerging countries and smaller when tax changes at the sub-national level are exploited.

JEL classification codes: E60; H22; H25; J30

Keywords: corporate income tax, tax incidence, wages, meta-regression analysis

6.1. Introduction

In its Final Report on Action 11, the Organisation for Economic Co-operation and Development (OECD) concludes that the *"economic incidence, particularly of the CIT in a global economy, is still an unresolved issue for economists"* (Organisation for Economic Co-operation and Development 2015, p. 116). Yet, the wage elasticity to corporate income tax (CIT) is frequently used to evaluate the effects of policy reforms (e.g., Council of Economic Advisers 2017; Watson and McBride 2021). In light of ambiguous evidence, advances on the issue are of considerable interest, since the tax incidence is a key parameter for policy makers due to its implications for the progressivity and distributive fairness of a tax system (Auerbach 2006).

The academic tax incidence discussion dates back at least to Harberger (1962). In a closed two-sector economy with fixed stocks of capital and labor, the CIT burden falls entirely on capital in the long run. Since the simple Harberger (1962) model abstracts from several important determinants, such as international capital flows, extensions assume an open economy in which capital is perfectly mobile across countries while labor is immobile (e.g., Mutti and Grubert 1985; Harberger 1995; Gravelle and Smetters 2006; Randolph 2006; Harberger 2008). Taxing capital induces capital flows to low-tax jurisdictions and a decline of the marginal labor productivity in the high-tax country, leading to lower wages.

Most empirical studies corroborate evidence of labor bearing a substantial share of the CIT, since their estimates suggest a tax incidence of 30–100% (e.g., Desai, Foley, and Hines 2007; Felix 2007; Arulampalam, Devereux, and Maffini 2012; Suárez Serrato and Zidar 2016; Fuest, Peichl, and Siegloch 2018; Dwenger, Rattenhuber, and Steiner 2019). A first stream of studies addresses the *indirect* tax incidence effect through capital reallocation across countries or states over time, that is, the open-economy general equilibrium mechanism. The first empirical study in this vein by Hassett and Mathur (2006) estimates extremely large wage elasticities, since their findings imply that a one dollar increase in corporate tax revenue is associated with a decrease in wages by 22 to 26 dollars, evaluated at the ratio of labor income to corporate tax revenue of 26.7 (Gravelle and Hungerford 2007). A second emerging strand of literature focuses on the *direct* tax incidence effect (e.g., Arulampalam, Devereux, and Maffini 2012; Moore 2014; Felix and Hines 2022). Extending the general wage bargaining model of McDonald and Solow (1981), Arulampalam, Devereux, and Maffini (2012) argue that firms and their employees bargain over the share of the firm's economic after-tax profit that is paid out as wages. Using firm-level data on 55,082 European firms from nine countries for the years 1996–2003, they estimate that a 1% increase in the CIT payment per employee results in a decrease in wages of 0.09% in the long run. Evaluated at the sample mean values, this elasticity estimate suggests that a one dollar increase in the tax liability tends to reduce wages by 49 cents.

Thus, the precise magnitude of the tax incidence remains controversial.[1] Why do these divergences emerge in the reported results? It is widely acknowledged among literature surveys that the inconsistency of tax elasticity estimates of wages is associated with differences in methodological aspects (e.g., the data or the estimation method) across studies. This paper contributes to the literature by using meta-regression analysis (MRA) to quantitatively combine the empirical CIT incidence literature.[2] First, we test for the presence of publication bias in the primary literature. Second, we investigate to what extent the estimates are driven by different study characteristics and methodological choices, to shed light on the sources of heterogeneity among reported effects. Third, we compute how much of the tax burden is borne by employees in the form of lower wages, on average. We go beyond a qualitative literature review by deriving *best-practice* effect estimates with respect to corporate taxes. To the best of our knowledge, we provide the first MRA to explain the inconclusiveness observed in the tax incidence literature.

Our results suggest that the primary studies suffer from substantial publication selection in favor of a wage-reducing effect of corporate taxes. After correcting for the bias by using a battery of correction techniques, we find no significant average relation between wage rates and corporate taxes in our statistical tests.

Next, we explain the variation of estimates by adding variables regarding the underlying tax variable, estimation technique, specification, and data set. First, the negative association between wages and corporate taxes originating from the tax variables that capture tax base–related incentives is stronger. Second, econometric modeling choices explain some of the heterogeneity among the estimates, while accounting for endogeneity does not seem to be a primary heterogeneity source. Third, studies exploiting tax changes at the federal level yield stronger responses than studies focusing on sub-national taxes. Fourth, our results reveal evidence that the wage effect is significantly different for studies using data from emerging countries. Lastly, the reported estimates tend to move towards zero over time, which might be due to growing corporate tax avoidance by multinational firms. Building on these MRA results, we construct a synthetic study with ideal methodological choices to compute the implied best-practice estimates. Once we simultaneously account for publication bias and heterogeneity sources, our baseline estimates indicate an average association that is very close to zero, confirming that the tax incidence effect is largely exaggerated by publication bias. Nevertheless, we find stronger negative wage

[1] Many studies point to this, e.g., Gentry (2007); Harris (2009); Dwenger, Rattenhuber, and Steiner (2019).

[2] Meta-analysis is a statistical tool for averaging estimates from a comparable strand of literature (Stanley and Doucouliagos 2012). Recent sophisticated meta-analyses related to key economic parameters include the study of Gechert et al. (2021) on the elasticity of substitution between capital and labor, the work of Gechert and Heimberger (2022) on the impact of taxes on economic growth, the study of Havránek et al. (2020) on the consumption response to income changes, and the study of Bajzik et al. (2020) on the elasticity of substitution between domestic and foreign goods.

responses to corporate tax changes at the federal level and in emerging countries, which is consistent with theoretical considerations.

The remainder of this paper is structured as follows. Section 6.2 briefly reviews the prior theoretical and empirical literature. Section 6.3 describes our data collection and standardization procedure and presents the meta-sample. Section 6.4 tests for the presence of publication bias. Section 6.5 discusses our variables for the heterogeneity tests, along with their descriptive statistics, and displays the MRA results and best-practice estimates. Finally, Section 6.6 concludes the paper by summarizing and discussing the implications of our results.

6.2. Brief review of literature

At least two theoretical mechanisms through which the CIT is possibly shifted onto employees in the form of reduced wages have guided the existing empirical literature: The indirect and the direct tax incidence effect.

General equilibrium concepts capture the effects of the CIT on wages via capital stock adjustments and price changes, that is, the *indirect* tax incidence effect. The seminal contribution by Harberger (1962) assumes a competitive two-sector closed economy with fixed stocks of capital and labor, while both factors are perfectly mobile across sectors. Under a set of reasonable parameter assumptions, he concludes that the tax burden fully falls on capital in the long run. First, a corporate tax on capital induces a substitution of labor for capital, thereby decreasing its rate of return relative to labor (factor substitution effect). Second, the production costs increase concurrently, depressing output and raising product prices in the corporate sector (product substitution effect). As a result, capital and labor move to the noncorporate sector, while the price of capital only declines if the corporate sector is relatively more capital intensive than the noncorporate one. Since the 1960s, the global economy has become increasingly more integrated. Several extensions of Harberger's early model therefore consider an open economy where capital is mobile across countries while labor is not (e.g., Mutti and Grubert 1985; Harberger 1995; Gravelle and Smetters 2006; Randolph 2006; Harberger 2008). In this setting, taxing capital results in capital flows abroad, a decline in labor productivity given factor complementarities, and ultimately lower wages. Assuming an open economy in which capital can escape higher taxation by moving overseas thus passes a part of the burden to the immobile factor labor.

To what extent does labor bear the burden of the CIT? After Harberger published his influential study in 1962, the following work has widely assessed this question by calibrating theoretical models with assumptions about the real-world economy. The key insight from the major theoretical open-economy studies is that tax incidence is driven by a few economic parameters, such as the degree of international capital mobility, substitutability between domestic and foreign products, and the size of the economy.[3] This literature predicts that imperfect product substitution reduces the ability to shift capital abroad, which decreases the share of the tax borne by employees. Conversely, the tax burden entirely falls on labor when the open economy is small relative to the rest of the world, because the worldwide rate of return to capital is not affected by a tax change in such a small country.

Since the theoretical evidence appears to be ambiguous, it is an empirical task to determine the extent to which capital and labor bear the burden of the CIT. Early papers explored the tax incidence by estimating the short-run effects of corporate tax changes on the rate of return to capital without providing explicit tax incidence estimates on labor (e.g., Krzyzaniak and Musgrave 1963; Cragg, Harberger, and Mieszkowski 1967; Gordon 1967; Dusansky 1972; Oakland 1972). Given the difficulty of cleanly separating tax shifting from other determinants of the rate of return to capital, more recent research investigates whether *wage rates* are responsive to corporate taxation (Gentry 2007). Building upon a general equilibrium model, the pioneering study in this vein by Hassett and Mathur (2006) uses aggregate wage data for 72 countries between 1981 and 2002 and estimates a long-run wage elasticity to the CIT of roughly -1.[4] Their estimates imply that a one dollar increase in corporate tax revenue would reduce wages by 22 to 26 dollars, which largely exceeds the range of plausible magnitudes (Gravelle and Hungerford 2007). This paper attracted much criticism as it, among other things, controls for value added per employee, which shuts down the impact through the open-economy general equilibrium mechanism. Felix (2007), Gravelle and Hungerford (2007), and Clausing (2012) replicate the model specifications of Hassett and Mathur (2006) and produce estimates with considerably smaller magnitudes. Felix (2007) considers individual household data for 19 developed OECD countries between 1979 and 2002 and finds no statistically significant association when she controls for the degree of openness of the economy. Gravelle and Hungerford (2007) note that the results of Hassett and Mathur (2006) are sensitive to several methodological choices, such as alternative exchange rate conversions and the use of five-year average wage rates. More recent versions of the paper integrate spatial effects by controlling for tax rates in neighboring countries and report less negative elasticity estimates of about -0.5 (Hassett and Mathur 2010; Hassett and Mathur 2015). Another often-cited paper by Desai, Foley, and Hines (2007) focuses on affiliates of US multinational firms across more than 50 countries in 1989, 1994, 1999, and 2004. Unlike the prior studies, they constrain the total incidence as a sum of the shares

[3] For a detailed discussion of the key assumptions of open-economy general equilibrium models see the review by Gravelle (2013).

[4] Note that the only earlier study in our meta-sample by Gyourko and Tracy (1989) does not primarily assess the tax incidence but rather tests for the broad presence of compensating wage differentials across cities generated by variation in fiscal conditions such as state corporate tax rates.

of labor and capital to unity, and estimate a tax incidence of 45-75% falling on labor.

In contrast to the papers surveyed above, a number of studies exploit variation in tax rules across US states over time (e.g., Carroll 2009; Liu and Altshuler 2013; Suárez Serrato and Zidar 2016; Ljungqvist and Smolyansky 2018). Their estimates are on the lower bound compared to the earlier findings by Hassett and Mathur (2006), emphasizing the role of relative capital and labor mobility in determining the final tax incidence. Suárez Serrato and Zidar (2016), for example, incorporate imperfect mobility of input factors by modelling location, supply, and demand decisions simultaneously while considering location-specific fixed productivity faced by firms. Accordingly, the firms' location decisions are not infinitely elastic in response to tax rate changes, since their achievable productivity varies across locations independent of the tax rate. Their empirical findings suggest that capital owners bear 40%, landowners 25-30%, and employees 30-35% of the tax burden.

A growing second body of literature argues that the tax incidence effect operates through a rent-sharing mechanism, which is referred to as the *direct* tax incidence effect. Conceptually, wage rates are set via efficient bargaining processes between firms and their employees. An increase in the tax rate reduces the firm's economic after-tax profit over which both parties bargain, leading directly to a decline in domestic wage rates (e.g., Riedel 2011; Arulampalam, Devereux, and Maffini 2012). According to the theoretical model of Arulampalam, Devereux, and Maffini (2012), the magnitude of this effect depends on the firm's relative bargaining power: the stronger the firm's power, the smaller the employees' share of the location-specific profit. Hence, the part of the tax that is shifted onto employees *decreases* with the firm's relative bargaining power. Riedel (2011) provides a reasoning why the predictions of the direct tax incidence mechanism might not be as clear-cut. Considering union wage bargaining within multinational firms, her model acknowledges that labor costs are deductible from the domestic taxable base, which reduces the effective tax burden of the firm. As the value of this tax shield increases with higher tax rates, the firm's after-tax profit becomes less responsive to labor costs, tending to increase domestic wages.

To identify the direct effect empirically, studies exploit cross- or within-firm (industry) variation in tax liabilities or rates. By controlling for pre-tax value added per employee, Arulampalam, Devereux, and Maffini (2012) claim to capture the direct tax incidence effect. They estimate that a 1% increase in the corporate tax payment per employee results in a decrease of wages by 0.093% in the long run, using firm-level data on 55,082 European firms from nine countries from 1996 to 2003. However, their setting is prone to endogeneity, since the exploited variation in tax liabilities across countries is not solely driven by tax policy reforms but is also affected by firms' investment decisions and other confounding variables. Several other studies rely on within-country data to mitigate these identification concerns, suggesting that parameter estimates are less likely to be biased by changing economic conditions and other unobserved factors (e.g., Felix and Hines 2022; Moore 2014; Bauer, Kasten, and Siemers 2017; Dwenger, Rattenhuber, and Steiner 2019).

Taken together, the common theoretical prediction underpinning empirical studies is a wage-decreasing effect of corporate taxation, which implies that the CIT is partly shifted on labor and thus tax cuts would actually benefit employees. The large variety of methodological aspects across studies, such as estimation and data choices, may result in conflicting conclusions about wage responses to corporate taxes. It also has been pointed out that the determined share of the tax burden that is passed onto employees may depend on whether a study exploits variation of sub-national rather than federal taxes; if labor productivity is controlled for; and which country (groups) are investigated. We attempt to shed light on these considerations in our quantitative MRA in the next sections.

6.3. Meta-sample

6.3.1. Data collection

MRA results are only meaningful if the estimates are comparable across primary studies (Stanley 2001). The wage elasticity to corporate taxes is commonly estimated according to the following generic regression equation:

$$log(wage) = \theta + \gamma \cdot log(CIT) + \omega \cdot X + \epsilon \tag{6.1}$$

where $wage$ is the wage rate, CIT denotes the CIT variable (see Section 6.5 for further variable descriptions), the vector X typically contains various country-, individual-, or firm-level controls, and ϵ is the error term. The main coefficient γ (expected to be negative) captures the corporate tax elasticity of wages and is the outcome variable of our MRA.[5] The tax elasticity of wages determines the percentage change in wages if the tax variable increases by 1%, that is, $\delta log(wage)/\delta log(CIT)$.

We collected all studies that estimate a variant of Eq. (6.1) described above. Our paper follows the recent guidelines of Havránek et al. (2020) for conducting meta-analyses. We conducted a comprehensive search process to locate appropriate studies using search engines and other sources. In Appendix , we describe our search strategy and selection

[5] Suárez Serrato and Zidar (2016), Fuest, Peichl, and Siegloch (2018), Ljungqvist and Smolyansky (2018), and Dyreng et al. (2022) use a net-of-tax rate. Desai, Foley, and Hines (2007), Agarwal and Chakraborty (2017), and Karuppiah and Shanmugam (2022) use a constrained net-of-tax rate. Moore, Kasten, and Schmidt (2014), Kakpo (2021), Li, Wu, and Zheng (2020), and Harju, Koivisto, and Matikka (2022) exploit tax cuts in a (generalized) difference-in-differences (DiD) setting. We therefore multiply their estimates by -1.

process in more detail. Figure A.6.1 presents a PRISMA flow chart which illustrates the literature selection steps (Moher et al. 2009), and Table A.6.1 lists the selection of primary studies. In sum, 31 studies match our selection criteria. Since selecting a single estimate per study is quite subjective and results in less heterogeneity among estimates, we include multiple estimates from each primary study, as long as there is a considerable difference between the variables, estimation methods, model specifications, or samples.

6.3.2. Standardization

Some primary studies use other versions of Eq. (6.1) besides a log-log specification. Moreover, the definitions of the CIT variables vary across studies. We therefore convert the selected estimates into common metrics to ensure comparability. The standardization procedure is described in more detail in Appendix . We start by adjusting estimates that stem from interaction terms. The corresponding standard errors are approximated by applying the delta method. We then consistently transform estimates into partial correlation coefficients (PCCs), as follows (Stanley and Doucouliagos 2012):

$$PCC_{is} = t_{is}/\sqrt{t_{is}^2 + df_{is}} \tag{6.2}$$

where t_{is} is the t-statistic of the main coefficient γ of regression i of primary study s, and df_{is} is the regression's degrees of freedom. The corresponding standard error of the PCC is computed as $SE\,PCC_{is} = \sqrt{(1 - PCC_{is}^2)/df_{is}}$. The PCC captures the direction and significance level of the association between the CIT and wages.

Since the PCC is more of a statistical measure, we use the initial tax elasticity estimate as an alternative dependent variable, allowing for an economically meaningful interpretation. We transform deviating estimates into elasticities, using the reported sample average of the tax and wage rate variable. The t-statistics of the elasticity values remain the same, since the associated standard errors are computed in the same way. Finally, we use the standardization procedure proposed by Gechert and Heimberger (2022) to correct for the different sizes of elasticity estimates when, for example, the effective tax rate instead of the statutory tax rate is used. We rely on PCCs, as described above, as our preferred estimates to consider the largest possible set of existing studies.

6.3.3. Distribution of estimates

Table 6.1 provides an overview of the distribution of the PCCs and elasticity estimates in our final meta-sample. The estimates and corresponding standard errors are winsorized at the first and 99th percentiles to minimize the impact of outliers.[6] We additionally drop five estimates referring to the publication of Ebrahimi and Vaillancourt (2016), one estimate of Agarwal and Chakraborty (2017), and three estimates of Karuppiah and Shanmugam (2022), since those estimates are far to the left of the remainder of the funnel plot in Figure A.6.2 in Appendix due to their extremely low standard errors and can thus be considered outliers (Stanley and Doucouliagos 2012).[7] To ensure comparability, we have to exclude 103 estimates for the subset of elasticity estimates. Table A.6.2 in Appendix gives an overview of the excluded estimates, along with the reason for exclusion. After these adjustments, our subsets of PCCs and elasticity estimates comprise 484 and 381 observations, respectively.

Table 6.1: Distribution of estimates

	N	Avg.	Median	Min	Max	Std. dev.
PCCs	484	-0.062	-0.020	-0.518	0.050	0.106
Elasticity estimates	381	-0.217	-0.083	-1.203	0.293	0.286

Note: This table provides an overview of the distribution of the PCCs and elasticity estimates. The estimates and corresponding standard errors are winsorized at the first and 99th percentiles.

The values of the PCCs vary far above -1.0 and below +1.0, thereby scattering much less compared to the elasticity estimates; the (unweighted) average is -0.062, with a standard deviation of 0.106. Since the mean value ranges between -0.037 and -0.076, the meta-sample exhibits a *medium* correlation between corporate taxes and wages, on average (see Doucouliagos 2011, p. 14, Table 4, field "politics and taxes"). The mean value of the tax elasticity of wages equals -0.217, with a standard deviation of 0.286. Accordingly, the wage rate decreases by 0.22% if the tax rate increases by 1%, on average. The minimum and maximum illustrate a left-skewed distribution due to large negative values.

6.4. Testing for publication bias

6.4.1. Graphical evidence

Publication bias arises if statistically nonsignificant or supposedly counterintuitive estimates are not published in a journal or do not even appear in a working paper due to certain preferences of authors, editors, and reviewers. To visually test for the presence of publication bias, the funnel plot in Figure 6.1 maps the PCCs to their precision, that is, the inverse of the standard error (Egger et al. 1997). In absence of publication bias, the

[6]Our MRA results are robust to not winsorizing estimates and standard errors and winsorizing estimates and standard errors at the fifth and 95th percentiles (see Table C.6.3 in Appendix).

[7]We drop estimates with absolute values of the t-statistic above 64.

estimates should spread randomly around the average *true* effect (in our case, a zero effect; see Table 6.2, column (2)). The plot shows an elongated left tail missing the right side, since most estimates vary between -0.2 and zero, while positive estimates are almost absent. Moreover, 286 estimates are significant at least at the 5% level ($t \geq 1.96$), while 198 PCCs are not significant. The peak is composed of the most precise estimates, scattered around -0.02 and zero. The asymmetric shape of the funnel plot is more consistent with the presence of publication selection for the sign of the estimate than for statistical significance, because the funnel is thick rather than hollow (Havránek, Irsova, and Zeynalova 2018).[8] Accordingly, estimates with positive values are selected less often for publication than they should, which would bias our estimate of the average effect size. Since positive estimates contradict the predominant theoretical prediction (see Section 6.2), the authors could consider positive results a signal for model misspecification and adjust their models to produce statistically significant negative results. Figure 6.2 documents the density for the corresponding t-statistics of the PCCs ($t_{is} = |e_{is}|/SE_{is}$). The plot suggests a jump above the critical t-score of 1.96, providing graphical evidence of p-hacking.

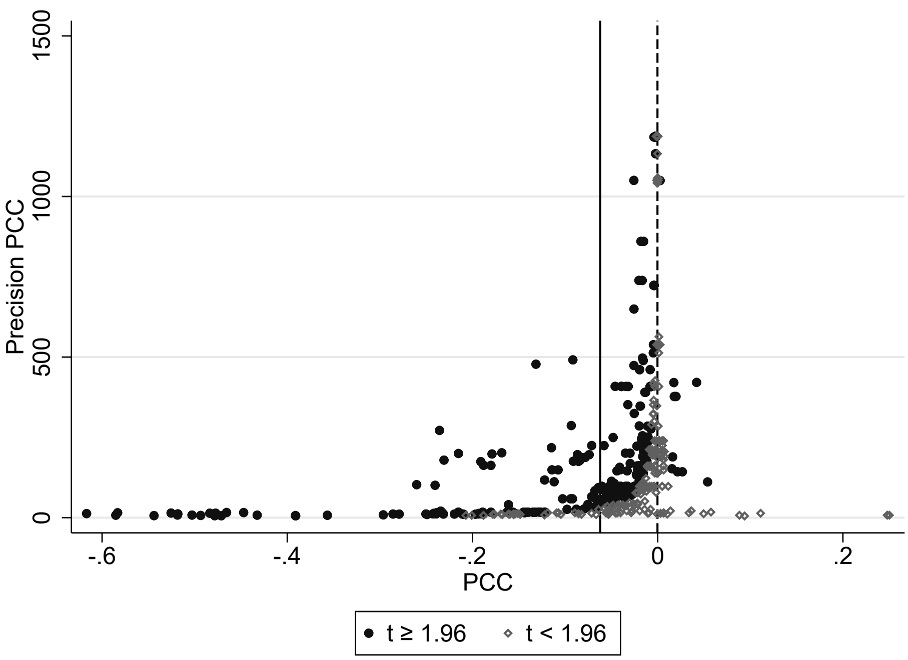

Figure 6.1: Funnel plot of the PCCs

Note: This funnel plot maps the PCC ($N = 474$) against its inverse of the standard error. For convenience of presentation, estimates with a precision above 1,500 are excluded from the figure but included in all statistical tests. Black crosses indicate significant estimates at least at the 5% level ($t \geq 1.96$), and gray rhombs indicate nonsignificant ones ($t < 1.96$). The dotted vertical line marks the weighted average (-0.000). The solid vertical line marks the unweighted average (-0.062).

[8]The funnel becomes hollow when nonsignificant estimates are omitted.

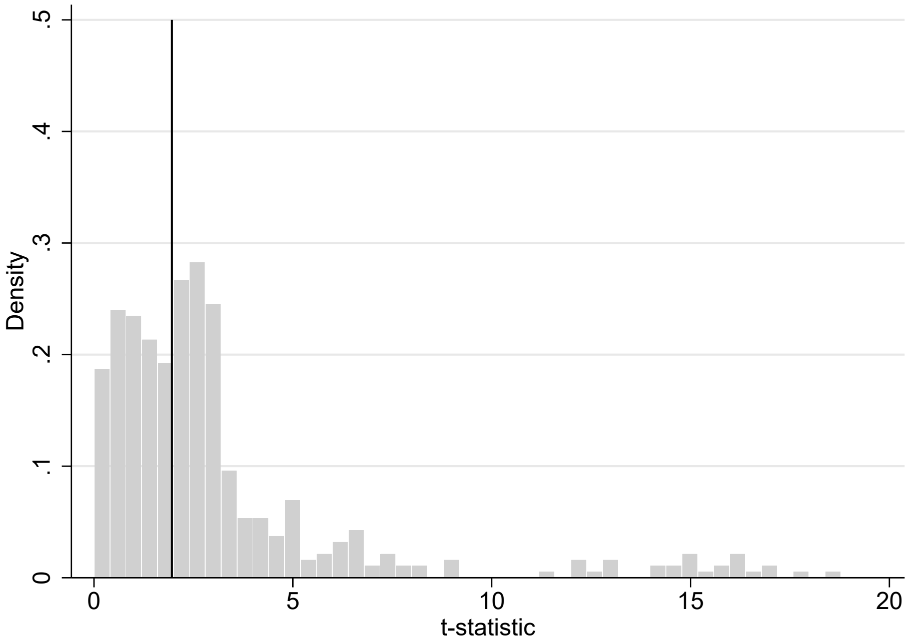

Figure 6.2: Density of the t-statistics of the PCCs

Note: This plot presents the density of the corresponding t-statistics ($t_{is} = |e_{is}|/SE_{is}$) of the PCCs ($N = 469$). For convenience of presentation, estimates with a t-statistic of $t_{is} > 20$ are excluded from the figure but included in all statistical tests. The vertical line marks the t-score of $t = 1.96$.

6.4.2. Formal tests

We address the issue of publication bias more formally by including the standard error of the corresponding estimate as an explanatory variable in the following regression:

$$PCC_{is} = \alpha + \beta \cdot SE\,PCC_{is} + \epsilon_{is} \tag{6.3}$$

where the dependent variable PCC_{is} is the PCC of regression i of primary study s, $SE\,PCC_{is}$ is the corresponding standard error, and ϵ_{is} is the error term. The funnel asymmetry test (FAT) for the coefficient on $SE\,PCC_{is}$ (β) detects the presence of publication bias (Egger et al. 1997). The underlying intuition is simple: a correlation between PCC_{is} and $SE\,PCC_{is}$ appears to be due to publication bias, because the authors could be searching for the expected sign or statistical significance by testing various estimation methods or model specifications under the given conditions, such as a small sample size, resulting in increased (decreased) values of estimates (standard errors). The precision effect test (PET) on the constant (α) assesses whether an average true effect beyond publication bias exists (Stanley 2008).

To correct for heteroscedasticity, Eq. (6.3) is weighted by the inverse of the PCC's variance ($1/SE\,PCC_{is}^2$), that is, weighted least squares (WLS) (Stanley and Doucouliagos 2015). Beyond correcting for heteroscedasticity, weighting by the inverse variance corrects for low-quality estimates, since imprecise coefficients are given less weight in the MRA. We consider multiple estimates from each primary study in our meta-sample, which carries the risk of within-study dependency (i.e., autocorrelation). We allow for autocorrelation between the estimates per primary study due to unobserved study-level heterogeneity, and we cluster standard errors at the study level (Stanley and Doucouliagos 2012).

Table 6.2: Testing for publication bias

Variables	Avg.	FAT–PET	OLS	#	IV	Elast.
	(1)	(2)	(3)	(4)	(5)	(6)
Standard error (publication bias)		-3.689***	-1.959***	-4.874**	-2.109***	-3.434***
		(1.054)	(0.528)	(1.931)	(0.635)	(0.852)
Constant (average effect)	-0.062***	-0.000	-0.011	-0.001	-0.007	-0.001
	(0.018)	(0.001)	(0.008)	(0.002)	(0.010)	(0.001)
Number of observations	484	484	484	484	484	381
Adj. R-squared	0.000	0.152	0.432	0.094	0.430	0.150
Wild bootstrap CI			[-3.413, -0.861]			
F-Statistic					199.58	
					(0.000)[a]	
Anderson–Rubin CI					[-3.441,-0.022]	

Note: The dependent variables are the PCC, winsorized at the first and 99th percentiles in columns (1)–(5), and the elasticity estimate, winsorized at the first and 99th percentiles in column (6). Column (1) reports the unweighted average. WLS with the inverse of the squared standard error, winsorized at the first and 99th percentiles as weights, are used in columns (2) and (6); OLS are used in column (3) and the corresponding wild bootstrap confidence interval (CI) is reported; WLS with the inverse of the number of estimates times the squared standard error, winsorized at the first and 99th percentiles as weights, are used in column (4); and the inverse of the square root of the number of observations as the instrumental variable for the standard error is used in column (5) and the corresponding Anderson–Rubin confidence interval (CI) is reported. Standard errors are in parentheses and clustered at the study level. *** and ** indicate significance level of 0.01 and 0.05, respectively. [a]The p-value is reported.

Table 6.2 reports the results for testing publication bias in the literature. Column (1) contains the unweighted average PCC tested against zero, which serves as a reference estimate. We check whether our result in column (2), where WLS is used to correct for heteroscedasticity, is robust to alternative models and weights. Column (3) presents the results of a simple ordinary least squares (OLS) regression and reports the wild bootstrap confidence interval. Column (4) uses the inverse of the number of estimates per study times the squared standard error as an alternative weighting factor. Weighting by the number of estimates assigns a similar weight to all studies and corrects for the over- and underrepresentation of studies in the sample. Column (5) employs the inverse of the square root of the number of observations as an instrumental variable for the standard error, since some estimation methods can inherently produce larger estimates and corresponding standard errors (Havránek 2015). The exclusion restriction is satisfied by the argument that the number of observations of the estimate relates to its standard error and not to methodological choices. The first-stage F-statistic indicates that the number of observations is not weakly correlated with the standard error, rendering the variable a suitable instrument. To check whether the PCC transformation impacts our estimation results, column (6) turns to the subset of elasticity estimates.[9]

[9]Figure B.6.1 in the Appendix displays the funnel plot for the subset of elasticity estimates, providing

The negative ($\beta < 0$) and statistically significant parameter estimates on the variable *Standard error* are consistent with the presence of publication bias in favor of a wage-reducing effect of corporate taxation. After correcting for publication bias, we cannot distinguish the average tax incidence effect from zero, since the constants are negative but negligible in size and not statistically significant across our models (2) to (6). In a robustness test in Table C.6.2, we investigate whether the extent of publication bias is associated with a study's publication status or average sample year. We find that the issue of publication bias has become more prevalent over time and is less pronounced in papers published in peer-reviewed journals.

Table 6.3: Other publication bias correction techniques

Variables	Avg.	A–K	Stem	WAAP
	(1)	(2)	(3)	(4)
Average effect	-0.062***	0.015	-0.001	-0.002**
	(0.018)	(0.011)	(0.010)	(0.001)
Number of observations	484	484	3	60

Note: The dependent variable is the PCC, winsorized at the first and 99th percentiles. The unweighted average in column (1) serves as a reference estimate. The selection model of Andrews and Kasy, 2019 (A–K) with a critical z-score cutoff at 2.58 is used in column (2); the stem-based method of Furukawa, 2019 (Stem) is used in column (3); and the weighted average of adequately powered (WAAP) method of Ioannidis, Stanley, and Doucouliagos, 2017 is used in column (4). Standard errors are in parentheses and clustered at the study level, except for column (2). *** and ** indicate significance levels of 0.01 and 0.05, respectively.

To test the robustness of our results, Table 6.3 presents several other publication bias correction techniques applied in the recent meta-analytical literature (e.g., Bajzik et al. 2020; Gechert and Heimberger 2022). As before, column (1) contains the unweighted average PCC tested against zero. Column (2) shows the result of the selection model of Andrews and Kasy (2019), which computes an estimate's publication probability conditional on its z-statistic, thereby detecting jumps in the distribution of z-values just above critical z-scores. Figure 6.3 illustrates the publication probabilities of PCCs relative to negative estimates with a significance level of at least 1%. Publishing a significant negative result at the 5% and 10% level is 8.2 to 5.5 times more likely, while a positive and significant result at the 5% and 1% level is only 7.2% to 2.1% as likely, respectively.[10] The last two columns of Table 6.3 focus on various subsets of the most precise estimates. Column (3) applies the stem-based bias correction method by exploiting the trade-off between bias and variance (Furukawa 2019). Column (4) contains the WAAP method of Ioannidis, Stanley, and Doucouliagos (2017), which focuses on estimates with a power of at least 80%. To have adequate power, an estimate's standard error must be smaller than

graphical evidence of publication bias. In Table C.6.1, we provide evidence that our results in column (6) of Table 6.2 are robust to alternative models and weights.

[10]Note that the inferences about publication probabilities remain unchanged across parameter choices, while the estimate for the average true effect is sensitive to model specifications.

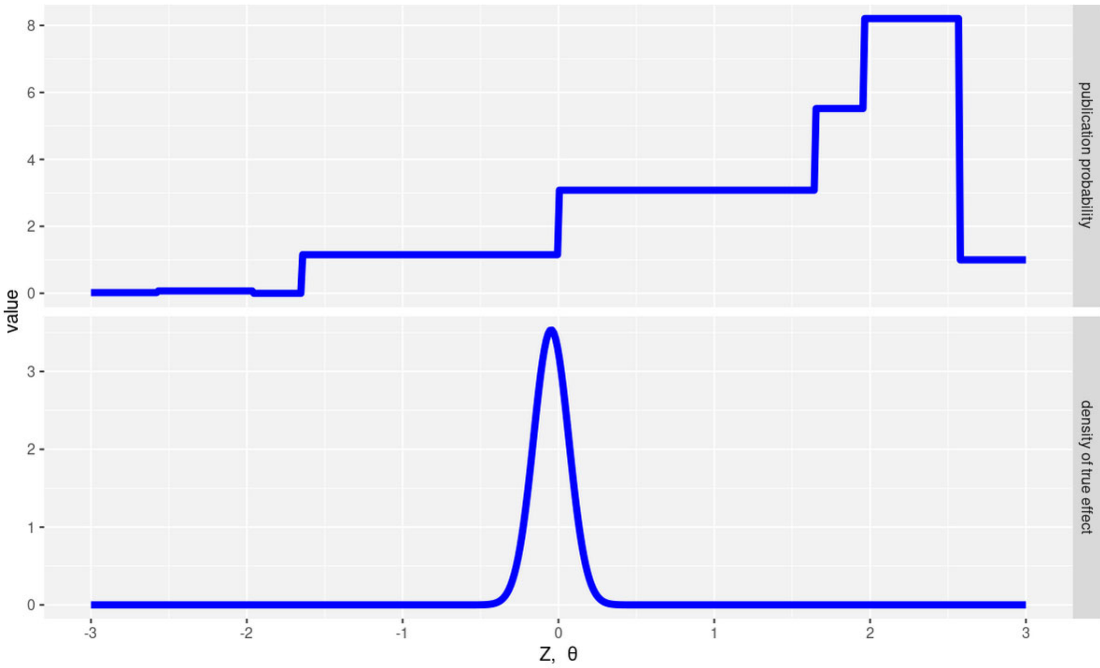

Figure 6.3: Publication probabilities from Andrews and Kasy (2019)

Note: The top panel displays the publication probabilities of PCCs ($N = 484$) depending on their z-statistics when a normal distribution is assumed and critical scores of 1.65, 1.96, and 2.58 as the thresholds are chosen. Note that the underlying PCCs are multiplied by -1 in order to set negative estimates as the reference category: negative estimates with a significance level of at least 1% are assigned a publication probability of one. Publishing a negative and statistically significant result at the 10% level is 5.5 times more likely. Publishing a negative but nonsignificant result is about 3.1 times more likely, while a positive and nonsignificant result is 1.2 times more likely. A positive and significant result at the 10%, 5%, or 1% level has a publication probability of zero, 7.2%, and 2.1%, respectively.
The bottom panel shows the density of the true average effect after publication bias correction.
Source: We use the web app available at `https://maxkasy.github.io/home/metastudy/`.

the average true effect divided by 2.8.

Taken together, we find robust evidence of substantial publication bias in the tax incidence literature. After accounting for publication bias, our results suggest a rather weak negative average wage response to corporate taxes that cannot be distinguished from zero in the vast majority of our statistical tests. The following section investigates how different study characteristics and methodological choices shape the average association between wages and corporate taxation.

6.5. Explaining heterogeneity

6.5.1. Sources of heterogeneity

We expand our baseline Eq. (6.3) by adding several dummy variables that capture the diverse characteristics and methodological choices of primary studies. Table 6.4 provides summary statistics for the full set of our explanatory variables, along with their description. The depicted mean values can be interpreted in percentage terms times 100. At first glance, the summary statistics point to heterogeneous estimates of the relevant literature, since the means vary sharply between the moderator variables.

Tax variables: Table 6.5 provides an overview of the CIT variables used in the underlying literature. Most of the estimates correspond to the top marginal statutory tax rate (STR) in a country, state, or municipality as the explanatory variable (56%), followed by *ex post* rates (26%), while only 4% apply *ex ante* rates, that are, the effective marginal tax rate (EMTR) and the effective average tax rate (EATR). The remaining estimates relate to indicator variables which assess the impact of an exogenous adjustment of the corporate tax rate affecting only one (treatment) group of firms.[11] The variables differ in their capability to incorporate tax base–related incentives, such as tax deductions, tax credits, and tax planning. While the STR and the tax rate change indicators neglect tax base provisions, ex ante rates capture the incentive to invest by assuming a mix of assets, financing sources, and fixed rates of interest, inflation, and depreciation allowances. Ex post rates represent the tax liability as a share of taxable profit or employees, thereby capturing both tax base provisions and tax planning.

The tax incidence effect might be sensitive to the applied tax variable. We distinguish only between the STR (plus most of the tax reform indicators) and the other measures by coding the variable *Tax base* to ensure enough variation.[12] We expect more

[11] To employ the same unit of measurement for indicator and continuous variables, we correct for the magnitude of coefficients stemming from indicator variables. For example, for Harju, Koivisto, and Matikka (2022), we approximate elasticities by acknowledging that the Finnish tax reform 2012-2014 reduced the top statutory CIT rate by about 23.07%.

[12] The study of Moore, Kasten, and Schmidt (2014) focuses on the 2000 German Business Tax Reform

Table 6.4: Summary statistics of variables

Variables	**Description**	**Summary statistics** (N = 484)	
		Mean	Std. dev.
Publication bias			
Standard error	PCC standard error, winsorized at the first and 99th percentiles	0.026	0.036
Tax variables			
STR	=1 if the explanatory variable is the STR, and 0 otherwise	0.560	0.497
Reform indicator	=1 if the explanatory variable is a tax reform indicator, and 0 otherwise	0.143	0.350
Ex ante rate	=1 if the explanatory variable is the EMTR or EATR, and 0 otherwise	0.039	0.194
Ex post rate	=1 if the explanatory variable is tax liability per profit or the tax liability per employee, and 0 otherwise	0.258	0.438
*Tax rate**	=1 if the explanatory variable neglects tax base provisions, and 0 otherwise	0.678	0.468
Tax base	=1 if the explanatory variable incorporates tax base provisions, and 0 otherwise	0.322	0.468
Estimation techniques and data			
*FE estimator**	=1 if the FE estimator is used, and 0 otherwise	0.438	0.497
Endogeneity	=1 if the DiD approach, RD design, IV, or GMM are used, and 0 otherwise	0.256	0.437
Other methods	=1 if the RE estimator or MLE are used, and 0 otherwise	0.039	0.194
SUR	=1 if the SUR is used, and 0 otherwise	0.087	0.282
OLS	=1 if OLS are used, and 0 otherwise	0.087	0.282
WLS	=1 if WLS are used, and 0 otherwise	0.093	0.291
Short-run effect	=1 if a short-run effect, and 0 otherwise	0.192	0.394
Long-run effect	=1 if a long-run effect, and 0 otherwise	0.052	0.222
*Static effect**	=1 if a static effect, and 0 otherwise	0.748	0.435
*Unconditioned**	=1 if a control for labor productivity, the capital–labor ratio, or capital is not included, and 0 otherwise	0.436	0.496
Conditioned	=1 if a control for labor productivity, the capital–labor ratio, or capital is included, and 0 otherwise	0.564	0.496
*National**	=1 if variation in national taxes is exploited, and 0 otherwise	0.490	0.500
Sub-national	=1 variation in state-, municipality-, or city-level taxes are exploited, and 0 otherwise	0.434	0.496

continues on next page

Country coverage			
*Advanced country**	=1 if the sample comprises a advanced country, and 0 otherwise	0.579	0.494
Emerging country	=1 if the sample comprises a emerging country, and 0 otherwise	0.081	0.272
Mix of countries	=1 if the sample comprises a mix of advanced and emerging countries, and 0 otherwise	0.287	0.453
Publication status			
*Working paper**	=1 if published as an academic working paper, and 0 otherwise	0.481	0.500
Published	=1 if published in a peer-reviewed journal, and 0 otherwise	0.457	0.499
Policy paper	=1 if published as a policy paper, and 0 otherwise	0.062	0.241
Time trend			
Average sample year	Average sample year, normalized between 0 and 1	0.582	0.186

Note: This table summarizes the full set of variables, along with a description and summary statistics for the subset of PCCs. The means of the variables times 100 can be interpreted in percentage terms. The superscript * marks the benchmark category of the respective study characteristics.

Table 6.5: Tax variables

Tax variables	**Tax base**	**Tax planning**
Statutory tax rate (STR)		
Reform indicator	(✓)	
Effective marginal tax rate (EMTR)	✓	
Effective average tax rate (EATR)	✓	
Effective tax rate (ETR)	✓	✓

imprecise estimates when using the STR, since it abstracts from effects of tax credits, incentives, and tax planning on the tax burden faced by firms. A consideration of the tax base is important because of two reasons. First, countries often justify higher CIT rates with more generous tax incentives. Second, the incentive to lower profits through tax planning increases with higher tax rates (Shevlin, Shivakumar, and Urcan 2019). Hence, the tax base and the tax rate are often not uncorrelated, even though this is implicitly assumed by studies.

Estimation techniques and data: We define four categories regarding the estimation techniques and data used to produce the estimates: (i) regression methods; (ii) temporal dynamics; (iii) control variables; and (iv) the exploited tax variation. More than 40% of the estimates are estimated by fixed effects (FE) estimators, which determine changes in wages over time while controlling for unobserved but fixed heterogeneity between countries, states, or individuals. Of the remainder, 25% attempt to correct for endogeneity by using DiD or regression discontinuity (RD) designs, instrumental variable (IV) procedures, and generalized method of moments (GMM) approaches. Other publications apply random effects (RE) estimators or maximum likelihood estimations (MLE). Seemingly unrelated regressions (SURs) aim to account for the cross-correlation of error terms between capital and labor regression equations and are most often estimated by generalized least squares (e.g., Exbrayat and Geys 2016; Agarwal and Chakraborty 2017). We expect a bias for simple OLS and WLS regressions that do not try to overcome endogeneity concerns.

The pass onto employees following a change of the CIT via capital reallocation and the adjustment of factor prices is likely to occur over time rather than immediately (Auerbach 2006). Consequently, the long-run effect should be greater than the short-run effect. Most studies, however, provide single-point estimates by using static models and are thus unable to cleanly address temporal dynamics. Since the exact distinction between short- and long-run estimates is ambiguous in some studies, we only mark an estimate as *Long-run effect* when the authors explicitly provide long-run estimates.[13] All other estimates are designated as *Short-run effect* or *Static effect*.

A number of studies, including Felix (2007), Gravelle and Hungerford (2007), and Hassett and Mathur (2015), rely on aggregate wage data and exploit tax rate changes across countries over time. Another stream of research uses, instead, changes in tax rates across states or municipalities within a single country (e.g., Suárez Serrato and Zidar 2016; Fuest, Peichl, and Siegloch 2018). Since primary studies exploit either variation in federal or sub-national taxes, we group studies into two clusters and add the moderator

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that reduced the top statutory CIT rate by about 26% while this tax rate reduction was accompanied by a broadening of the tax base. We therefore group their estimates under the variable *Tax base*.

[13]Hassett and Mathur (2015) justify their use of five-year average wage rates by noting that the economic effects of CIT rate changes show up over longer time periods due to capital adjustment costs; even so, we label their estimates as a static effect.

variable *Sub-national*. When considering corporate taxation in a within-country setting, complete immobility of the factor labor is a strong assumption. We therefore expect a smaller effect for the latter group, because labor is arguably more mobile within a country than across countries and can therefore more easily escape the tax burden of sub-national taxes by moving to other states or municipalities. Additionally, federal tax changes are commonly larger in magnitude than changes at the sub-national level, which could trigger a larger response of wages.

Building upon the discussion of the theoretical channels in Section 6.2, we attempt to disentangle the direct tax incidence effect by coding the variable *Conditioned*, which equals one if a study controls for labor productivity (e.g., value added per employee or the gross domestic product per employee), the total factor productivity, capital–labor ratio, or capital, and zero otherwise. By controlling for labor productivity, the impact of the CIT on wages via capital adjustments is controlled for, such that only the direct effect should remain. We are not able to separate the indirect tax incidence effect because the included primary studies either estimate reduced-form effects of the CIT on wages or do not report implied elasticities when two-step estimation procedures are used.

Country coverage: We explore whether the impact of the CIT on wage rates varies across countries with different levels of economic development. To test this, we distinguish among a sample of *Emerging countries*, *Advanced countries*, and a *Mix of countries*. On the one hand, a smaller tax incidence effect for emerging economies can be expected, because existing trade and capital restrictions could prohibit free capital flows, which decreases the share of the tax burden falling on labor. On the other hand, we predict a larger tax incidence effect on labor in emerging economies, because more potentially substitutable products may exist abroad for these countries due to better production technologies in advanced economies (Bajzik et al. 2020).

Publication status: We code whether an estimate is published in a peer-reviewed journal, an academic working paper, or a policy paper series of think tanks.

Time trend: Given an increasing global capital mobility during the last decades (e.g., Altshuler, Grubert, and Newlon 2001; De Mooij and Ederveen 2008; Feld and Heckemeyer 2011), the wage response to corporate taxes is likely to trend upward over time. As Clausing (2012) and Clausing (2013) notes, however, the capital mobility effect is mitigated by the growing importance of corporate tax avoidance via, e.g., profit shifting, since the CIT deploys a smaller impact when firms can shift paper profits to low-tax jurisdictions without corresponding real activity adjustments. Moreover, the conceptual model of Arulampalam, Devereux, and Maffini (2012) predicts that multinational firms shift a smaller share of the tax burden onto employees compared to their domestic peers, because they are able to reduce the location-specific profit over which both parties bargain by shifting income abroad. Consistent with this notion, Dyreng et al. (2022) document that the tax incidence falling on employees and the degree of tax avoidance by firms are negatively correlated. We code the variable *Average sample year*, normalized between zero and one, by setting the oldest average sample year (1979) to zero and the latest (2013) to one. Figure 6.4 illustrates the association between the PCCs and the average sample year by studies exploiting variation in national or sub-national taxes. We observe two conditions: First, studies exploiting taxes at a national and sub-national level are equally distributed over time. Second, the PCCs trend slightly upward over time (solid line), consistent with a mitigating impact of corporate tax avoidance on the final tax incidence on wages.

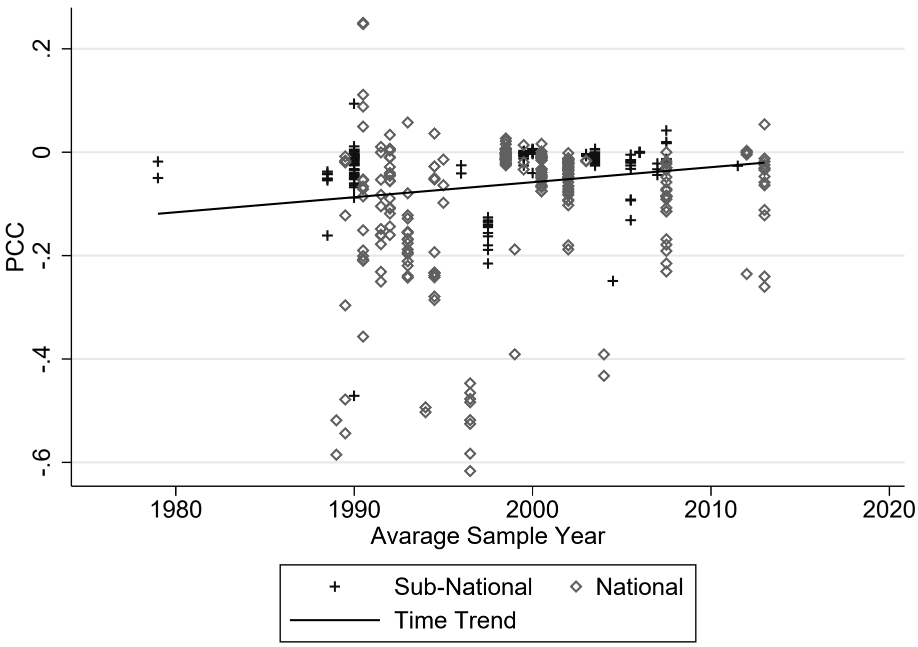

Figure 6.4: Time trend of the PCCs

Note: This plot illustrates the association between the PCCs ($N = 484$) and the average sample year, not normalized by studies exploiting tax variation at the sub-national and national levels. Black crosses indicate estimates exploiting sub-national tax variation, and gray rhombs indicate estimates exploiting national tax variation. The solid line indicates the time trend of the PCCs.

Table 6.6 investigates the degree to which PCCs vary with various methodological aspects, across countries, and over time by estimating Eq. (6.3). Instead of adding the full set of moderator variables simultaneously, we include them step by step to address multicollinearity concerns. Column (1) contains our baseline model. Column (2) adds methodological aspects of estimates. Column (3) further includes the moderators regarding the publication status, while column (4) adds the variables *Emerging country* and *Mix of countries* to explore how the underlying sample composition shapes the tax incidence effect on wages. Finally, column (5) includes the variable *Average sample year* to investigate whether the sample period is associated with the estimate. PCCs capture the strength of the association between the CIT and wages in terms of statistical significance, but do not allow for interpretation of the effect size. Column (6) therefore uses the elasticity estimate as the dependent variable. It should be noted that the value of the constant in Table 6.6 is conditioned on the included variables when they take zero values (i.e., they depend on the reference categories) and thus cannot be interpreted as an average effect.

Most importantly, the coefficients on the variable *Standard error* remain statistically significant at the 1% level across all columns. Adding the full set of moderator variables depresses the magnitude of the coefficient, but the evidence of substantial publication bias remains.

Regarding the methodological choices, we obtain the following: (i) The estimates for the variable *Tax base* are negative and statistically significant across all columns. We find evidence that the CIT variable's ability to capture tax base–related incentives systematically leads to a stronger negative association between corporate taxes and wages. (ii) Accounting for endogeneity does not seem to be a source of heterogeneous estimates, since the variable *Endogeneity* is mostly positive but not significant across the columns. However, using WLS instead of an FE estimator produces, on average, a less negative correlation, while the reverse is true for SUR estimates. (iii) Interestingly, controlling for labor productivity or capital yields more negative effect-size estimates, contradicting the theoretical considerations. We note that the difference is very small and not statistically significant across all columns. (iv) The positive coefficient estimates for *Sub-national* are consistent with our prediction that labor can escape the CIT burden of sub-national taxes more easily, leading to a smaller share of the tax falling onto employees.

Our estimates indicate a strong association between the publication status of a primary study and its reported results. The estimates reported in peer-reviewed journals and policy paper series are more negative in magnitude than those of working papers. While the peer-reviewed estimates are only marginally smaller, estimates taken from policy papers are largely different. Consistent with our expectations above, we find that the coefficients on the variable *Emerging country* are negative and statistically significant, which corroborates the determining influence of the product substitutability. The tax incidence effect on wages tends to decrease over time because the coefficients on *Average sample year* are positive across the columns, however, only significant in column (6). This matches the plot in Figure 6.4 and could be due to growing corporate profit shifting by multinational firms over the last decades (see, e.g., Wier and Zucman 2022), making wage rates less sensitive to higher domestic tax burdens.

Table C.6.3 in Appendix tests whether the main heterogeneity results of our preferred model in column (5) are sensitive to alternative model specifications. We present results of a simple OLS regression, the inverse of the number of estimates per study times the squared standard error as an alternative weighting factor, not winsorizing estimates and corresponding standard errors, and winsorizing estimates and corresponding standard

Table 6.6: Sources of heterogeneity

Variables	Baseline	+ Est.	+ Pub.	+ Coun.	+ Time	Elast.
	(1)	(2)	(3)	(4)	(5)	(6)
Publication bias						
Standard error	-2.462***	-1.981***	-1.989***	-1.927***	-1.919***	-3.163***
	(0.577)	(0.317)	(0.300)	(0.302)	(0.315)	(0.497)
Tax variables						
Tax base	-0.030**	-0.013***	-0.013**	-0.013***	-0.013***	-0.011
	(0.014)	(0.005)	(0.005)	(0.005)	(0.005)	(0.008)
Estimation techniques and data						
Endogeneity		0.009	0.010	0.011	0.011	-0.055
		(0.006)	(0.007)	(0.008)	(0.008)	(0.046)
Other methods		0.010*	0.011**	0.013*	0.013	-0.026
		(0.005)	(0.005)	(0.008)	(0.008)	(0.106)
SUR		-0.099***	-0.098***	-0.078***	-0.079***	-0.055
		(0.021)	(0.021)	(0.020)	(0.020)	(0.046)
OLS		-0.013	-0.000	-0.000	-0.000	-0.071
		(0.009)	(0.002)	(0.002)	(0.002)	(0.047)
WLS		0.018***	0.019***	0.014**	0.014**	-0.036
		(0.004)	(0.005)	(0.005)	(0.005)	(0.021)
Long-run effect		0.008*	0.008	0.008	0.007	0.008
		(0.005)	(0.005)	(0.005)	(0.005)	(0.008)
Short-run effect		-0.001	-0.002	-0.002	-0.002	0.008
		(0.005)	(0.005)	(0.006)	(0.006)	(0.008)
Conditioned		-0.000	-0.001	-0.002	-0.002	-0.003***
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Sub-national		0.012***	0.012**	0.015**	0.015**	0.004***
		(0.004)	(0.005)	(0.007)	(0.007)	(0.001)
Publication status						
Published			-0.000	-0.001	-0.001	-0.027***
			(0.002)	(0.001)	(0.001)	(0.004)
Policy paper			-0.027***	-0.028***	-0.028***	-0.118**
			(0.002)	(0.002)	(0.002)	(0.046)
Country coverage						
Emerging country				-0.018**	-0.018***	-0.014**
				(0.006)	(0.006)	(0.007)
Mix of countries				0.008	0.008	-0.001
				(0.007)	(0.007)	(0.035)
Time trend						
Average sample year					0.001	0.017**
					(0.005)	(0.007)

continues on next page

Constant	-0.000	-0.012***	-0.012**	-0.014**	-0.014*	0.057
	(0.001)	(0.004)	(0.005)	(0.007)	(0.008)	(0.046)
Number of observations	484	484	484	484	484	381
Adj. R-squared	0.266	0.516	0.568	0.575	0.574	0.596

Note: The dependent variables are the PCC, winsorized at the first and 99th percentiles in columns (1)–(5), and the elasticity estimate, winsorized at the first and 99th percentiles in column (6). Detailed descriptions of our moderator variables are provided in Table 6.4. WLS with the inverse of the squared standard error, winsorized at the first and 99th percentiles as weights, are used. Standard errors are in parentheses and clustered at the study level. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

errors at the top and bottom 5% percentiles. The results support our estimates reported in column (5) of Table 6.6.[14]

6.5.2. Best-practice estimates

After correcting for publication bias, our results suggest a rather weak negative average wage response to corporate taxation that cannot be distinguished from zero. Nevertheless, our MRA pointed out that the average association is significantly driven by several methodological choices, such as the tax variable and regression method used, exploited tax variation, and country coverage. Since our paper aims to provide consensus estimates implied by the relevant tax incidence literature, we use the reported results in column (5) of Table 6.6 to calculate *best-practice* estimates as a linear combination of the constant and the coefficient estimates. Similar to Havránek, Irsova, and Zeynalova (2018), we construct a representative best-practice study by building on the prior theoretical and empirical literature discussed in the last sections. Ideally, a study should incorporate tax base–related incentives, deal with endogeneity, and undergo a peer-review process as a quality indicator. To calculate our baseline best-practice estimate, we set variables consistent with the best practice to one, variables that depart from the best practice and the variable *Standard error* to zero, and all other variables to their sample mean. In a second step, we account for systematical heterogeneity in the relevant literature by setting the indicated variables to one, leaving everything else equal to the baseline. As the tax incidence effect can be interpreted more thoroughly if elasticity estimates are used, we re-run our calculations using the reported results of the elasticity specification in column (6) of Table 6.6.

Table 6.7 reports the best-practice estimates, along with their 95% confidence intervals. The baseline estimates indicate an average association that is very close to zero, thereby confirming the notion that the tax incidence effect is largely exaggerated by pub-

[14] To address model uncertainty, Table C.6.4 shows the results of Bayesian model averaging (BMA) for our most comprehensive model in Table 6.6, column (5). The results are in line with our main heterogeneity results. Note that the BMA was conducted without clustering standard errors at the study level. When we run regressions with the BMA variables and cluster standard errors at the study level, we obtain similar results to Tables 6.6 and C.6.3.

Table 6.7: Best-practice estimates by sources of heterogeneity

	PCCs		Elasticity estimates	
	Mean	95% CI	Mean	95% CI
Baseline	-0.011	[-0.022;0.001]	-0.025	[-0.041;-0.008]
Unconditioned	-0.010	[-0.022;0.002]	-0.024	[-0.040;-0.007]
Conditioned	-0.012	[-0.023;-0.000]	-0.026	[-0.043;-0.010]
National	-0.017	[-0.027;-0.008]	-0.027	[-0.043;-0.010]
Sub-national	-0.003	[-0.020;0.015]	-0.023	[-0.039;-0.007]
Advanced country	-0.012	[-0.023;-0.001]	-0.023	[-0.031;-0.014]
Emerging country	-0.030	[-0.044;-0.015]	-0.037	[-0.052;-0.022]

Note: This table provides best-practice estimates of the PCCs and elasticity estimates, along with 95% confidence intervals (CI), implied by the coefficient estimates in columns (5) and (6) of Table 6.6, respectively. Detailed descriptions of our moderator variables are provided in Table 6.4.

lication bias in prior studies. In sum, our average effect estimates suggest that the tax incidence effect on wages is economically small. Using our best-practice average elasticity as a reference point, a 1% increase in the CIT rate is associated with a decline in wages of only 0.025%, suggesting that the average effect after correcting for publication and heterogeneity bias seems to be only a fraction of the unweighted average of prior studies. Moreover, the best-practice estimates implied by the significant sources of heterogeneity confirm our previous conclusions regarding the association between wages and corporate taxation: we find more negative elasticity estimates among emerging economies and less negative elasticity estimates if sub-national tax variation is exploited.

6.6. Concluding discussion

Over the past 15 years, studies have provided empirical evidence of labor bearing a substantial share of the CIT through lower wages. Since these findings hinge on different methodological choices, the exact magnitude of the tax incidence is still ambiguous. This paper sets up a comprehensive meta-sample containing 31 studies to investigate the following: (1) How large is the average wage response to corporate taxes? (2) What are the sources of heterogeneity among estimates? We contribute to the literature by evaluating the impact of heterogeneity of diverse study characteristics and by providing implied best-practice estimates to shed light on the inconclusiveness of the empirical CIT incidence literature.

While the unweighted mean of the tax elasticity estimates of wages is -0.217, we find robust evidence of substantial publication bias in favor of a wage-reducing effect of the CIT. After correcting for the bias, our results indicate an average association that is indistinguishable from zero. The heterogeneity analysis reveals that the estimated wage effect depends on how the corporate tax burden is measured, which econometric method is used, the exploited tax variation, and the data coverage. More precisely, CIT variables that incorporate tax base effects lead to a stronger association between corporate taxes and wages. Not addressing endogeneity does not drive the heterogeneity systematically, while applying SUR (WLS) results in stronger (weaker) associations. The degree of shifting is much stronger for studies exploiting variation in federal taxes and using data for emerging countries but lower in more recent years, which might be due to growing corporate tax avoidance by multinational firms. Our best-practice estimates confirm these results: simultaneously accounting for publication bias and heterogeneity sources leads to a baseline estimate that is very close to zero, while the average association is the smallest for estimates stemming from sub-national tax variation and the largest for emerging countries. This is reminiscent of product substitution effects shaping the incidence outcomes, labor being more mobile within than across countries, and federal tax changes being more substantial, on average, than sub-national reforms.

Our MRA indicates that the degree of shifting tends to be larger when primary studies control for labor productivity, contradicting theoretical considerations. A reason for not finding a consistent effect could be that some of the primary studies are ambiguous about the theoretical justification for their econometric model. For example, Hassett and Mathur (2015) motivate their specifications with open-economy models on the indirect tax incidence effect, even though they control for labor productivity. As controlling for labor productivity does not seem to be sufficient to cleanly separate the direct tax incidence effect, it is advisable for future research to further investigate the underlying theoretical mechanisms and coherently derive the corresponding empirical specifications.

We also acknowledge some limitations of our paper. First, the meta-sample covers estimates from very different studies. Since our sample with 31 studies is rather small, we are not able to control for all study characteristics. To consider the largest possible set of existing studies, we include estimates based on indicator and continuous tax variables. Although we account for different units of measurement by approximating the impact of a 1% change if indicator variables are used, more insights could be gained by using a multi-treatment MRA approach as more primary studies become available. Second, the underlying tax incidence mechanism is very complex. The economic interactions of various factors, such as the size of an economy (e.g., Hassett and Mathur 2015), employment effects (e.g., Dwenger, Rattenhuber, and Steiner 2019), or the degree of tax competition (e.g., Liu and Altshuler 2013; Hassett and Mathur 2015) could have an impact on the estimated tax incidence effect. Therefore, part of the conclusions of the literature remains unexplored in our MRA. Our results should thus be interpreted with conventional caution, but they are a good starting point.

Despite its limitations, the tax incidence literature is often used to assess policy reforms. For advocates of lower corporate taxes, a large tax incidence on wages is a very appealing argument. In line with this, the Council of Economic Advisers (2017) uses a conservative range of elasticity estimates from -0.16 to -0.33 to calculate an average annual advantage of $4,000 for employees when the CIT rate is lowered by 15 percentage points. At that time, this was a strong argument in favor of Donald Trump's Tax Cuts and Jobs Act, which introduced a flat rate of 21% for corporate profits in the United States in 2018.[15] Similarly, the Tax Foundation claims in its evaluation of President Joe Biden's corporate tax plan that *"studies examining corporate income taxes support the idea that employees bear a large portion of the corporate income tax through lower wages"* (Watson and McBride 2021, p. 11).[16] Considering the robust evidence of substantial publication bias and the small average effect estimates in our paper, the proposed incidence argument in favor of lower CIT rates should be made with care.

[15] The Washington Post (2017) and The Wall Street Journal (2017) criticized the Council of Economic Advisers' estimates as implausible, because they imply an increase in wages of three to five times the tax revenue loss. Gale and Haldeman (2021) provide evidence that wage growth has slowed after the reform was enacted.

[16] The evaluation was conducted by Watson and McBride on behalf of the Tax Foundation in February 2021. The Tax Foundation assumes in its analysis that 50% of the CIT burden is borne by employees, while this estimate is taken from Fuest, Peichl, and Siegloch (2018).

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Appendices

Appendix A: Studies included in the meta-sample

A.1 Search strategy and selection process

We used Google Scholar and the IDEAS database to search for studies that assess the impact of the CIT on wage rates. We employed the following keywords in the search process: *corporate tax AND wages*, *tax incidence AND wages*, and *tax elasticity AND wages*. We then checked the reference lists of the surveys by Gentry (2007), Harris (2009), and Gravelle (2011) and all previously selected studies. After screening the title and abstract of studies, a total of 311 records were retrieved. We inspected the full-text articles to eliminate those studies that did not match our selection criteria. Our final meta-sample is determined by the following selection criteria:

- *Variant of Eq. (6.1)*: The study must report results from estimating a variant of Eq. (6.1) described in Section 6.3.1, where the dependent variable is the wage rate and the explanatory variable a measure of the CIT as described in Section 6.5. This excludes studies not empirically investigating the association between corporate taxes and wage rates.
- *Information*: The study must provide coefficients, corresponding standard errors or associated t-statistics, and the number of observations. This requirement is necessary to calculate PCCs and to test for publication bias.
- *Language*: We only include studies written in the English language.
- *Publication date and type*: We only include studies published before September 2022. We exclude Masters and PhD theses (but working papers are included).
- *Latest version*: We consider only the latest version of a study to avoid autocorrelation among estimates.

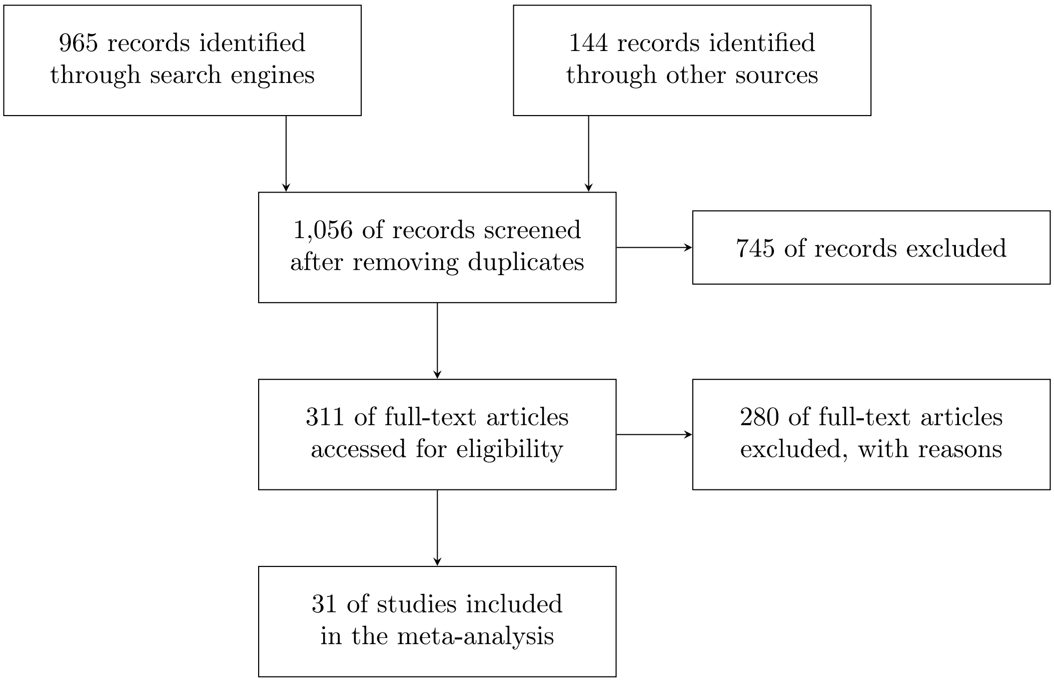

Figure A.6.1: PRISMA flow chart

Note: This PRISMA flow chart illustrates the selection steps of the literature (Moher et al. 2009).

Table A.6.1: Included primary studies

#	Authors and publication year	Tax variable	Period	Country
1	Agarwal and Chakraborty, 2017	Tax liability per profit (constrained)	2000–2015, 2011–2015	India
2	Arulampalam, Devereux, and Maffini, 2012	Tax liability per employee	1996–2005	European countries
3	Moore, 2014	Tax liability per employee	1994–2010, 1994–2007	France, UK
4	Moore, Kasten, and Schmidt, 2014	Tax reform indicator	1996–2005	Germany
5	Azémar and Hubbard, 2015	STR	1980–2004	OECD countries
6	Bauer, Kasten, and Siemers, 2017	STR	1995–2004	Germany
7	Becker, Fuest, and Riedel, 2012	STR	2000–2006	European countries
8	Carroll, 2009	STR	1970–2007	US
9	Clausing, 2012	STR, tax liability per profit	1981–2009, 1981–2008, 1990–2009	OECD countries
10	Desai, Foley, and Hines, 2007	Tax liability per profit (constrained)	1989–2004	OECD countries
11	Dwenger, Rattenhuber, and Steiner, 2019	Tax liability per profit	1998–2006	Germany
12	Dyreng et al., 2022	STR	1998–2016	US
13	Ebrahimi and Vaillancourt, 2016	STR	1998–2013	Canada
14	Exbrayat and Geys, 2016	STR	1982–2007	OECD countries
15	Felix, 2007	STR, tax liability per profit	1979–2002	OECD countries
16	Felix and Hines, 2022	STR	2000	US
17	Fuest, Peichl, and Siegloch, 2018	STR	1999–2008	Germany
18	Goodspeed, 2014	STR	2003	US
19	Gravelle and Hungerford, 2007	STR, EMTR, EATR	1981–2002	Worldwide
20	Gyourko and Tracy, 1989	STR	1979	US
21	Harju, Koivisto, and Matikka, 2022	Tax cut indicator	2008–2016	Finland
22	Hassett and Mathur, 2015	STR, EMTR, EATR	1981–2005	Worldwide
23	Kakpo, 2021	Tax cut indicator	2000–2015	US
24	Karuppiah and Shanmugam, 2022	Tax liability per profit (constrained)	2005–2019, 2005–2008, 2009–2019	India
25	Li, Liu, and Sun, 2021	STR	2010–2013	China

continues on next page

26	Li, Wu, and Zheng, 2020	Tax cut and hike indicators	1990–2007	OECD countries
27	Liu and Altshuler, 2013	EMTR	1982–1997	US
28	Ljungqvist and Smolyansky, 2018	STR	1970–2010	US
29	McKenzie and Ferede, 2017	STR, EMTR	1981–2014, 1997–2012	Canada
30	Misra, 2019	STR	2000–2012	Germany
31	Suárez Serrato and Zidar, 2016	STR	1980–2012	US

Note: This table outlines the full meta-sample of primary studies, with an overview of the underlying tax variable, sample period, and data coverage. STR = Statutory tax rate; EMTR = Effective marginal tax rate; and EATR = Effective average tax rate.

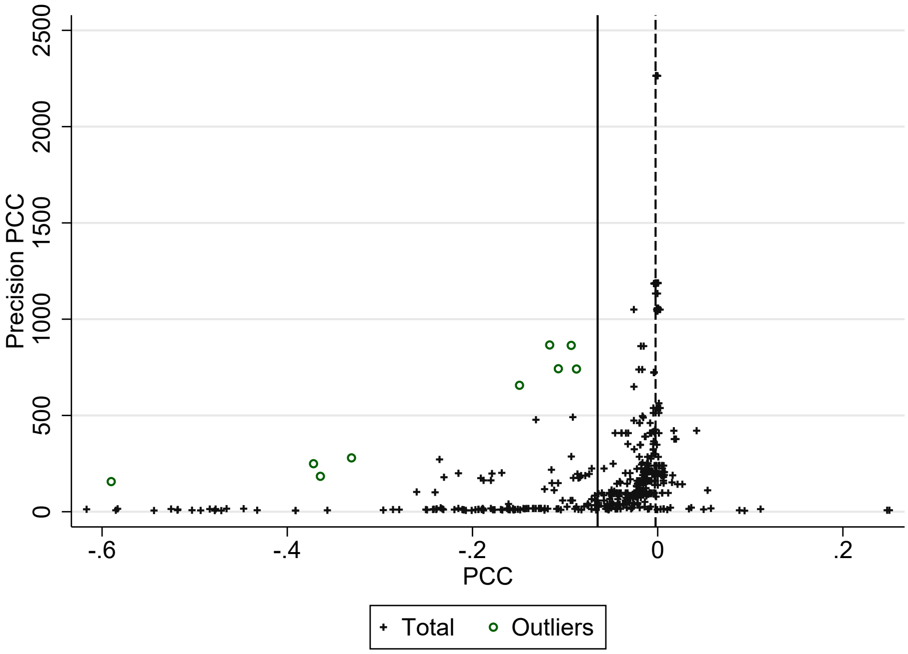

Figure A.6.2: Funnel plot of the PCCs with outliers

Note: This funnel plot maps the PCCs against their inverse of the standard error ($N = 493$). The dotted vertical line marks the weighted average (-0.002). The solid vertical line marks the unweighted average (-0.064). The nine outliers refer to the publications of Ebrahimi and Vaillancourt (2016), Agarwal and Chakraborty (2017), and Karuppiah and Shanmugam (2022).

A.2 Standardization procedure

To adjust estimates that stem from interaction terms, we calculate the average marginal effect of corporate taxes on wage rates by adding up the reported regression coefficients of the tax variable and the interaction term times the sample mean value of the variable included in the interaction term, that is, $coef_{CIT} + coef_{IT} \cdot mean(IT)$. We follow Cazachevici, Havránek, and Horvath (2020) and apply the delta method to approximate the corresponding standard errors by assuming the covariances to be zero, that is, $SE = \sqrt{SE_{CIT}^2 + SE_{IT}^2 \cdot mean(IT)^2}$. We exclude a respective estimate when the study does not provide the mean value of the interaction variable, which is needed to derive the average marginal effect (e.g., Azémar and Hubbard 2015; Harju, Koivisto, and Matikka 2022.

Some studies use vector autoregression models that produce a multitude of estimates that are not independent of each other (i.e., Gravelle and Hungerford 2007; Azémar and Hubbard 2015; Arulampalam, Devereux, and Maffini 2012; Moore 2014; Dwenger, Rattenhuber, and Steiner 2019; Kakpo 2021). We therefore include only short-run estimates in t and long-run estimates that are derived from the lagged coefficients of the vector autoregression processes, if reported.

We then standardize estimates across primary studies by calculating PCCs. The calculation of PCCs (Table B.6.1) requires an estimate's t-statistic, the number of observations, and the degrees of freedom. We use the number of observations of Hassett and Mathur (2006) for the estimates of Gravelle and Hungerford (2007), since they use the same data. We compute t-statistics by dividing the regression coefficient by its standard error ($t = coef/SE$). We assume that the standard errors of Karuppiah and Shanmugam (2022) are at most 0.0004, since they report standard errors of 0.000. We calculate the degrees of freedom by subtracting the number of considered variables in a regression (e.g., controls as well as firm or country dummies) from the number of observations minus one.

To provide economically meaningful estimates, we transform initial regression coefficients into elasticity estimates where necessary. Starting with the initial sample of 484 estimates taken from 31 primary studies in Table B.6.1, we have to exclude 103 estimates for the subset of elasticity estimates (Table B.6.3) to ensure comparability. Table A.6.2 gives an overview of the excluded estimates, along with the reason for exclusion.

We use the reported (estimation) sample average of the tax and wage rate variables to calculate elasticities, that is, $semi\,elasticity \cdot mean(CIT)$ or $coef \cdot mean(CIT)/mean$ $(wage)$. If only the mean value of the logarithm of the tax variable is reported (e.g., Gravelle and Hungerford 2007; Agarwal and Chakraborty 2017; Karuppiah and Shanmugam 2022), we approximate the mean value by $e^{mean(log(CIT))}$. For sub-samples, we apply the main sample average of the tax measure as approximation, if the estimation sample average is not reported. We use the reported mean values of Hassett and Mathur (2006) to derive the elasticity estimates of Gravelle and Hungerford (2007) and the METRs from 1997 to 2012 of Chen and Mintz (2006) and Chen and Mintz (2012) to calculate the mean value of the METR of McKenzie and Ferede (2017).

Finally, we correct for the size of the tax variables used in the primary studies, using the standardization procedure described by Gechert and Heimberger (2022). We calculate the ratio of STR/EATR when the EATR is used as the main explanatory variable, STR/EMTR when the EMTR is used, STR/tax liability per profit if tax liability per profit is used, and 1/percentage change of the STR if a tax reform dummy in a DiD setting is used. We use these ratios to standardize the elasticity and the corresponding standard errors.

Four estimates of Clausing (2012) are excluded, as she does not provide descriptive statistics for the tax liability per profit, which are needed to derive standardized elasticity values. We exclude some of the estimates of Ljungqvist and Smolyansky (2018) for the same reason. We exclude almost all estimates of the study by Li, Wu, and Zheng (2020), who do not provide descriptive statistics for the percentage change in the STR of the tax reforms. A complication of using elasticity estimates is that estimates are not directly comparable if the unit of measurement of the independent variable varies. Arulampalam, Devereux, and Maffini (2012) and Moore (2014) use the corporate tax liability per employee (absolute unit) instead of traditional tax rate measures (percentage unit). We therefore exclude these studies from the subset of elasticity estimates.

Table A.6.2: Estimates excluded from the subset of elasticity estimates

Authors and publication year	#	**Reasons for exclusion**
Arulampalam, Devereux, and Maffini, 2012	16	Tax liability per worker (absolute unit) is used as the explanatory variable.
Moore, 2014	37	Tax liability per worker (absolute unit) is used as the explanatory variable.
Clausing, 2012	4	Average value of the tax liability per profit is not reported.
Li, Wu, and Zheng, 2020	44	Percentage (points) change of the STR is not reported.
Ljungqvist and Smolyansky, 2018	2	Average value of the STR is not reported.

Appendix B: Supplementary statistics

B.1 PCCs

Table B.6.1: Descriptive statistics for PCCs

#	Authors and publication year	PCCs				
		N	Mean	Min	Max	Std. dev.
1	Agarwal and Chakraborty, 2017	37	-0.085	-0.260	0.050	0.073
2	Arulampalam, Devereux, and Maffini, 2012	16	-0.016	-0.046	0.016	0.017
3	Moore, 2014	37	-0.040	-0.188	-0.002	0.040
4	Moore, Kasten, and Schmidt, 2014	12	-0.059	-0.076	-0.034	0.014
5	Azémar and Hubbard, 2015	19	-0.053	-0.160	0.034	0.055
6	Bauer, Kasten, and Siemers, 2017	40	-0.003	-0.026	0.003	0.004
7	Becker, Fuest, and Riedel, 2012	3	-0.017	-0.018	-0.016	0.001
8	Carroll, 2009	6	-0.065	-0.161	-0.037	0.048
9	Clausing, 2012	12	-0.036	-0.128	0.050	0.058
10	Desai, Foley, and Hines, 2007	16	-0.461	-0.518	-0.188	0.085
11	Dwenger, Rattenhuber, and Steiner, 2019	9	-0.070	-0.103	-0.035	0.023
12	Dyreng et al., 2022	3	-0.033	-0.044	-0.022	0.011
13	Ebrahimi and Vaillancourt, 2016	13	-0.039	-0.131	-0.005	0.039
14	Exbrayat and Geys, 2016	9	-0.201	-0.286	-0.050	0.089
15	Felix, 2007	17	-0.095	-0.357	0.050	0.123
16	Felix and Hines, 2022	14	-0.001	-0.040	0.006	0.012
17	Fuest, Peichl, and Siegloch, 2018	39	-0.009	-0.026	0.006	0.009
18	Goodspeed, 2014	8	-0.004	-0.007	-0.004	0.001
19	Gravelle and Hungerford, 2007	11	-0.124	-0.250	0.010	0.086
20	Gyourko and Tracy, 1989	2	-0.034	-0.050	-0.018	0.022
21	Harju, Koivisto, and Matikka, 2022	5	-0.001	-0.005	0.003	0.003
22	Hassett and Mathur, 2015	15	-0.180	-0.243	-0.079	0.041
23	Kakpo, 2021	8	0.004	-0.019	0.042	0.023
24	Karuppiah and Shanmugam, 2022	1	-0.236	-0.236	-0.236	.
25	Li, Liu, and Sun, 2021	1	-0.026	-0.026	-0.026	.
26	Li, Wu, and Zheng, 2020	47	-0.005	-0.025	0.027	0.011
27	Liu and Altshuler, 2013	8	-0.185	-0.518	-0.008	0.216
28	Ljungqvist and Smolyansky, 2018	51	-0.033	-0.471	0.050	0.067
29	McKenzie and Ferede, 2017	14	-0.159	-0.249	-0.126	0.037
30	Misra, 2019	9	-0.000	-0.001	0.001	0.001
31	Suárez Serrato and Zidar, 2016	2	-0.033	-0.041	-0.025	0.011
	Total meta-sample	484	-0.062	-0.518	0.050	0.106

Note: This table provides descriptive statistics for the PCCs ($N = 484$, winsorized at the first and 99th percentiles) of the primary studies. The estimates of Desai, Foley, and Hines, 2007, Moore, Kasten, and Schmidt, 2014, Suárez Serrato and Zidar, 2016, Fuest, Peichl, and Siegloch, 2018, Harju, Koivisto, and Matikka, 2022, Ljungqvist and Smolyansky, 2018, Agarwal and Chakraborty, 2017, Dyreng et al., 2022, Kakpo, 2021, Karuppiah and Shanmugam, 2022, and Li, Wu, and Zheng, 2020 are multiplied by -1 (net-of-tax rate or DiD estimates).

Table B.6.2: Correlation matrix of variables

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
(1) *Standard error*	1.000															
(2) *Tax base*	0.153	1.000														
(3) *Endogeneity*	-0.122	0.537	1.000													
(4) *Other methods*	0.369	-0.003	-0.119	1.000												
(5) *SUR*	0.252	0.321	-0.181	-0.062	1.000											
(6) *OLS*	-0.145	-0.150	-0.181	-0.062	-0.095	1.000										
(7) *WLS*	-0.182	-0.221	-0.188	-0.065	-0.099	-0.099	1.000									
(8) *Long-run effect*	0.007	0.079	0.269	-0.047	-0.072	-0.072	-0.075	1.000								
(9) *Short-run effect*	-0.164	0.472	0.603	-0.099	-0.150	-0.094	-0.156	-0.114	1.000							
(10) *Conditioned*	-0.080	0.214	0.125	-0.166	-0.040	-0.084	0.281	-0.134	0.270	1.000						
(11) *Sub-national*	-0.363	-0.595	-0.304	-0.155	-0.255	0.278	-0.280	-0.110	-0.279	-0.483	1.000					
(12) *Published*	-0.066	-0.259	-0.101	-0.143	-0.165	0.101	0.349	0.180	-0.163	0.078	-0.158	1.000				
(13) *Policy paper*	0.106	-0.067	-0.151	-0.052	-0.079	0.316	-0.082	-0.060	0.049	-0.016	0.103	-0.236	1.000			
(14) *Emerging country*	-0.152	0.413	0.191	-0.060	0.394	-0.091	-0.095	-0.069	0.241	0.153	-0.244	-0.241	-0.076	1.000		
(15) *Mix of countries*	0.322	-0.037	-0.195	0.271	0.177	-0.179	0.504	-0.004	-0.113	0.236	-0.556	0.390	-0.163	-0.188	1.000	
(16) *Average sample year*	-0.487	0.265	0.359	-0.287	0.147	0.128	-0.015	0.072	0.283	0.108	-0.029	0.041	-0.074	0.512	-0.259	1.000

Note: This matrix shows the correlation of the variables for the subset of PCCs ($N = 484$). Table 6.4 provides the descriptions of the variables.

B.2 Elasticity estimates

Table B.6.3: Descriptive statistics for elasticity estimates

#	Authors and publication year	Elasticity estimates				
		N	**Mean**	**Min**	**Max**	**Std. dev.**
1	Agarwal and Chakraborty, 2017	37	-0.009	-0.042	0.006	0.012
2	Moore, Kasten, and Schmidt, 2014	12	-0.003	-0.005	-0.002	0.001
3	Azémar and Hubbard, 2015	19	-0.047	-0.199	0.063	0.067
4	Bauer, Kasten, and Siemers, 2017	40	-0.040	-0.332	0.034	0.058
5	Becker, Fuest, and Riedel, 2012	3	-0.172	-0.215	-0.144	0.037
6	Carroll, 2009	6	-0.122	-0.172	-0.013	0.058
7	Clausing, 2012	8	-0.250	-0.725	0.034	0.275
8	Desai, Foley, and Hines, 2007	16	-0.691	-0.826	-0.491	0.118
9	Dwenger, Rattenhuber, and Steiner, 2019	9	-0.963	-1.203	-0.701	0.186
10	Dyreng et al., 2022	3	-0.324	-0.338	-0.306	0.016
11	Ebrahimi and Vaillancourt, 2016	13	-0.168	-0.314	-0.037	0.071
12	Exbrayat and Geys, 2016	9	-0.098	-0.227	-0.009	0.082
13	Felix, 2007	17	-0.158	-0.498	0.293	0.301
14	Felix and Hines, 2022	14	0.014	-0.019	0.040	0.021
15	Fuest, Peichl, and Siegloch, 2018	39	-0.348	-1.203	0.293	0.256
16	Goodspeed, 2014	8	-0.050	-0.055	-0.042	0.004
17	Gravelle and Hungerford, 2007	11	-0.429	-0.836	0.020	0.299
18	Gyourko and Tracy, 1989	2	-0.065	-0.069	-0.062	0.005
19	Harju, Koivisto, and Matikka, 2022	5	-0.000	-0.001	0.000	0.000
20	Hassett and Mathur, 2015	15	-0.596	-0.821	-0.249	0.152
21	Kakpo, 2021	8	-0.001	-0.004	0.003	0.004
22	Karuppiah and Shanmugam, 2022	1	-0.038	-0.038	-0.038	.
23	Li, Liu, and Sun, 2021	1	-0.005	-0.005	-0.005	.
24	Li, Wu, and Zheng, 2020	3	-0.001	-0.001	-0.001	0.000
25	Liu and Altshuler, 2013	8	-0.280	-0.511	-0.083	0.145
26	Ljungqvist and Smolyansky, 2018	49	-0.339	-0.798	0.262	0.276
27	McKenzie and Ferede, 2017	14	-0.100	-0.150	-0.040	0.031
28	Misra, 2019	9	-0.005	-0.057	0.035	0.030
29	Suárez Serrato and Zidar, 2016	2	-0.991	-1.203	-0.780	0.299
	Total meta-sample	381	-0.217	-1.203	0.293	0.286

Note: This table provides descriptive statistics for the elasticity estimates ($N = 381$, winsorized at the first and 99th percentiles) of the primary studies. The estimates of Desai, Foley, and Hines, 2007, Moore, Kasten, and Schmidt, 2014, Suárez Serrato and Zidar, 2016, Fuest, Peichl, and Siegloch, 2018, Harju, Koivisto, and Matikka, 2022, Ljungqvist and Smolyansky, 2018, Agarwal and Chakraborty, 2017, Dyreng et al., 2022, Kakpo, 2021, Karuppiah and Shanmugam, 2022, and Li, Wu, and Zheng, 2020 are multiplied by -1 (net-of-tax rate or DiD estimates).

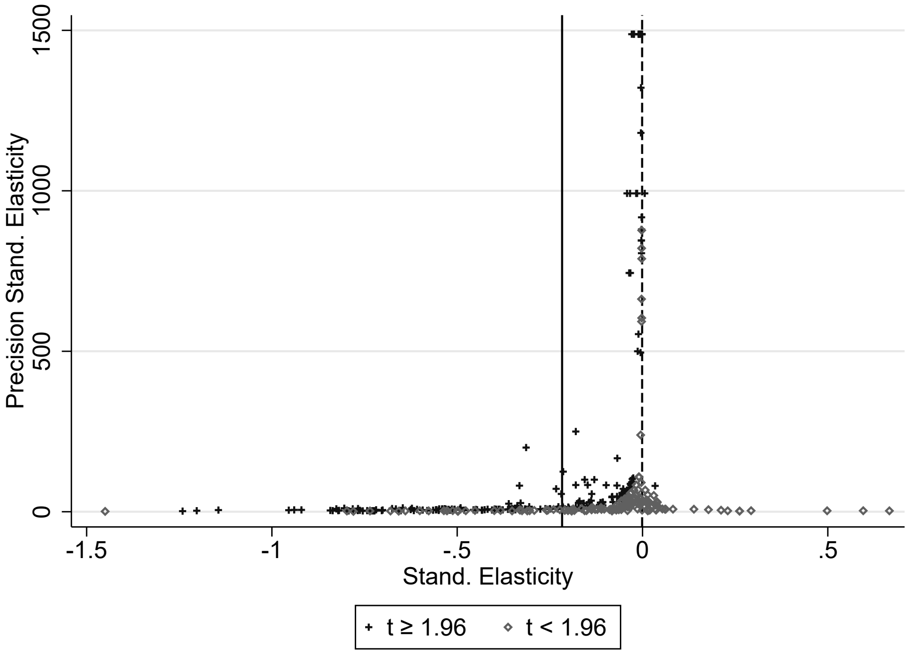

Figure B.6.1: Funnel plot of the elasticity estimates

Note: The funnel plot maps the elasticity estimate against its inverse of the standard error ($N = 347$). For convenience of presentation, estimates with a precision above 1,500 and below -3 are excluded from the figure but included in all statistical tests. Black circles indicate significant estimates at least at the 5% level ($t \geq 1.96$), and gray rhombs indicate nonsignificant ones ($t < 1.96$). The dotted vertical line marks the weighted average (-0.001). The solid vertical line marks the unweighted average (-0.217).

Appendix C: Robustness checks

Table C.6.1: Robustness: Testing for publication bias

Variables	Pref.	OLS	#	IV
	(1)	(2)	(3)	(4)
Standard error (publication bias)	-3.434***	-1.248***	-4.523***	-1.503*
	(0.852)	(0.254)	(1.382)	(0.908)
Constant (average effect)	-0.001	-0.074**	-0.001	-0.044
	(0.001)	(0.030)	(0.002)	(0.085)
Number of observations	381	381	381	381
Adj. R-squared	0.150	0.355	0.098	0.340

Note: The dependent variable is the elasticity estimate, winsorized at the first and 99th percentiles. Column (1) contains our preferred FAT–PET estimate. OLS are used in columns (2); WLS with the inverse of the number of estimates times the squared standard error as weights are used in column (3); and the inverse of the square root of the number of observations as the instrumental variable for the standard error is used in column (4). Standard errors are in parentheses and clustered at the study level. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

Table C.6.2: Drivers of publication bias

Variables	Published	Policy	Time	All
	(1)	(2)	(3)	(4)
Standard error	-5.076***	-3.520***	3.081	2.476*
	(1.746)	(1.099)	(1.904)	(1.413)
Standard error · Published	3.005			3.131**
	(1.835)			(1.311)
Standard error · Policy paper		0.589		-0.385
		(2.347)		(1.873)
Standard error · Average sample year			-12.326***	-13.392***
			(4.362)	(3.576)
Constant	0.000	0.000	-0.000	-0.009
	(0.003)	(0.001)	(0.009)	(0.007)
Number of observations	484	484	484	484
Adj. R-squared	0.179	0.234	0.221	0.314

Note: The dependent variable is the PCC, winsorized at the first and 99th percentiles. WLS with the inverse of the squared standard error, winsorized at the first and 99th percentiles as weights, are used. The variables *Published*, *Policy paper*, and *Average sample year* are included separately, but the coefficients are not reported. Standard errors are in parentheses and clustered at the study level. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.10, respectively.

Table C.6.3: Robustness: Sources of heterogeneity

Variables	Pref.	OLS	#	No Winsor	Winsor 5%
	(1)	(2)	(3)	(4)	(5)
Standard error	-1.919***	-1.772***	-1.884***	-1.904***	-1.784***
	(0.315)	(0.242)	(0.351)	(0.323)	(0.272)
Tax variables					
Tax base	-0.013***	-0.049*	-0.013**	-0.013***	-0.012**
	(0.005)	(0.026)	(0.006)	(0.005)	(0.005)
Estimation techniques and data					
Endogeneity	0.011	-0.008	0.013**	0.011	0.010
	(0.008)	(0.018)	(0.006)	(0.008)	(0.008)
Other methods	0.013	0.084***	0.010**	0.013	0.010
	(0.008)	(0.025)	(0.005)	(0.008)	(0.007)
SUR	-0.079***	-0.172***	-0.163***	-0.079***	-0.081***
	(0.020)	(0.042)	(0.028)	(0.020)	(0.018)
OLS	-0.000	-0.026**	0.000	-0.000	-0.001
	(0.002)	(0.012)	(0.002)	(0.002)	(0.002)
WLS	0.014**	-0.036**	0.015***	0.014**	0.011*
	(0.005)	(0.017)	(0.003)	(0.005)	(0.006)
Long-run effect	0.007	-0.003	0.004	0.007	0.005
	(0.005)	(0.011)	(0.005)	(0.005)	(0.005)
Short-run effect	-0.002	0.002	-0.002	-0.002	-0.006
	(0.006)	(0.014)	(0.006)	(0.006)	(0.006)
Conditioned	-0.002	0.022*	-0.002	-0.002	-0.001
	(0.001)	(0.012)	(0.003)	(0.001)	(0.001)
Sub-national	0.015**	-0.017	0.012***	0.015**	0.013*
	(0.007)	(0.026)	(0.004)	(0.007)	(0.007)
Publication status					
Published	-0.001	0.012	-0.003	-0.001	-0.001
	(0.001)	(0.011)	(0.002)	(0.001)	(0.001)
Policy paper	-0.028***	0.013	-0.029***	-0.028***	-0.027***
	(0.002)	(0.020)	(0.002)	(0.002)	(0.002)
Country coverage					
Emerging country	-0.018***	0.029	-0.021**	-0.018**	-0.018***
	(0.006)	(0.023)	(0.008)	(0.006)	(0.006)
Mix of countries	0.008	0.003	0.005	0.008	0.008
	(0.007)	(0.026)	(0.005)	(0.007)	(0.007)
Time trend					
Average sample year	0.001	0.018	0.001	0.001	0.003
	(0.005)	(0.026)	(0.015)	(0.005)	(0.004)
Constant	-0.014*	-0.006	-0.012	-0.014*	-0.015*
	(0.008)	(0.034)	(0.014)	(0.008)	(0.008)

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Number of observations	484	484	484	484	484
Adj. *R*-squared	0.574	0.708	0.842	0.572	0.577

Note: The dependent variable is the PCC, winsorized at the first and 99th percentiles. Detailed descriptions of our moderator variables are provided in Table 6.4. Column (1) contains our preferred model in Table 6.6, column (5); OLS are used in column (2); WLS with the inverse of the number of estimates times the squared standard error as weights are used in column (3); the PCC and standard error, not winsorized, are used in column (4); and the PCC and standard error, winsorized at the fifth and 95th percentiles, are used in column (5). Standard errors are in parentheses and clustered at the study level. ***, **, and * indicate significance levels of 0.01, 0.05, and 0.1, respectively.

Table C.6.4: Robustness: BMA for Table 6.6, column (5)

Variables	Post. mean	Post. std. dev.	PIP
	(1)	(2)	(3)
Constant (publication bias)	-1.882	NA	1.000
Precision	-0.000	0.002	0.068
Tax variables			
Tax base	-0.020	0.004	0.994
Estimation techniques and data			
Endogeneity	0.005	0.004	0.737
Other methods	0.000	0.003	0.051
SUR	-0.082	0.011	1.000
OLS	-0.000	0.001	0.047
WLS	0.001	0.003	0.113
Long-run effect	0.000	0.002	0.059
Short-run effect	-0.000	0.001	0.051
Conditioned	-0.000	0.000	0.048
Subnational	0.000	0.002	1.000
Publication status			
Published	0.000	0.000	0.045
Policy	-0.027	0.003	1.000
Country coverage			
Emerging country	-0.022	0.008	1.000
Mix of countries	0.000	0.001	0.080
Time trend			
Average sample year	0.000	0.001	0.053
Number of observations	484		

Note: The dependent variable is the PCC, winsorized at the first and 99th percentiles. Detailed descriptions of our moderator variables are provided in Table 6.4. All variables are weighted by the inverse of the estimate's *Precision* ($1/SE\,PCC_{is}$), winsorized at the first and 99th percentiles. Note that the constant in this model has to be interpreted as the estimate on the variable *Standard error* (publication bias). Column (1) contains the posterior mean coefficient estimates of variables; column (2) contains the corresponding standard errors; and column (3) depicts the posterior inclusion probability (PIP). All parameters were set in line with Havranek, Rusnak, and Sokolova, 2017. Standard errors are not clustered at the study level.

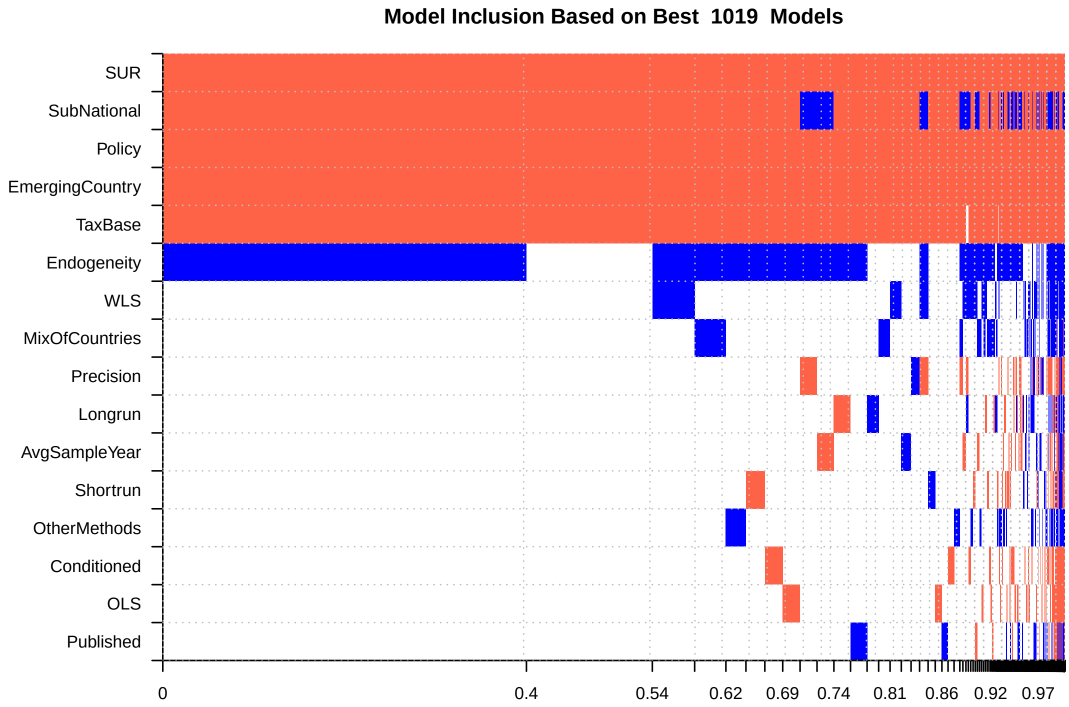

Figure C.6.1: Robustness: BMA for Table 6.6, column (5)

Note: This figure displays the results of applying Bayesian model averaging (BMA) technique. Inference in BMA is based on a weighted average of individual regressions that include different combinations of moderator variables; the weights reflect the posterior model probabilities (PMPs) of the individual specification (see Havranek, Rusnak, and Sokolova (2017) for further details). The vertical axis shows the moderator variables, ranked according to their posterior inclusion probability, and the horizontal axis shows the cumulative posterior probabilities for model inclusion. The blue color indicates a positive association between the variable and the PCCs, the red color indicates a negative association, and no color indicates that the variable is not included in the model. Table C.6.4 reports the corresponding estimation results. Note that the constant in this model has to be interpreted as the estimate on the variable *Standard error* (publication bias) in Table 6.6.